PROCEEDINGS

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THE PHYSICAL SOCIETY

OF LONDON.

February 1891.

XI. *On the Change in the Absorption-Spectrum of Cobalt Glass produced by Heat.* By Sir John Conroy, *Bt., M.A., Fellow and Bedford Lecturer of Balliol College, and Millard Lecturer of Trinity College, Oxford**.

Sir David Brewster made some experiments on the influence of heat on the absorbing power of coloured media, and states in his 'Treatise on Optics' (edit. 1853, p. 174) that he "was surprised to observe that it produced opposite effects upon different glasses, diminishing the absorbing power in some, and increasing it in others." He found that the transparency of a piece of purple glass was much increased on heating, whilst that of a yellowish-green glass and of a red glass was diminished; the purple glass recovered its colour on cooling, the other two did not do so completely.

Feussner (*Fortschritte der Physik*, 1867, p. 237) made some observations on the effect of heat on the absorption-spectra of substances in solution.

No observations on the effect of heat on the transparency of solid substances for rays of different refrangibilities except

* Read February 13, 1891.

those of Sir David Brewster appear to have been published, although, of course, the change of colour which borax blowpipe-beads containing certain metallic oxides undergo on cooling is well known. I therefore venture to communicate to the Physical Society some determinations which I have recently made on the changes produced by heat in the absorption-spectrum of cobalt glass.

The absorption-spectra of coloured glasses are not, as a rule, very characteristic, and merely show a continuous absorption extending through a considerable portion of the spectrum. Cobalt glass, however, as is well-known, has a characteristic absorption-spectrum consisting of three dark bands, in the red, yellow, and green, with a considerable amount of absorption between the first two; so that with a rather deeply coloured glass the transmitted light consists merely of the extreme red, some yellowish-green, and the blue and violet rays.

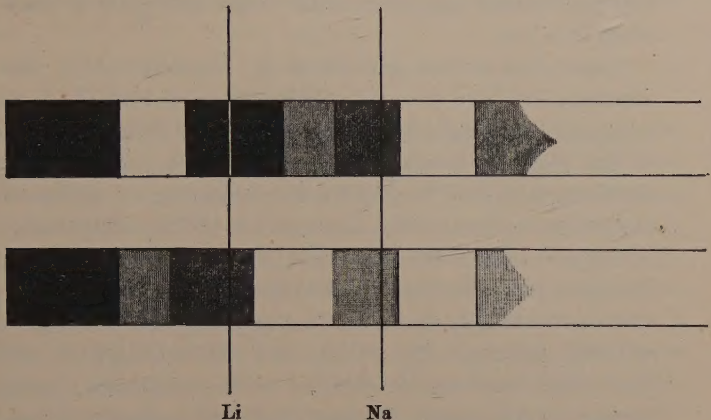
A small piece of this glass was heated by means of a medium-sized Bunsen (15 millim. tube), the glass being supported on, and nearly surrounded by, combustion-furnace tiles. It was found that in this way the glass could be heated till the edges began to soften and were visibly red, without much risk of its cracking either whilst being heated or during its subsequent cooling. An ordinary gas-jet was used as the source of light, and the light transmitted by the glass examined with a spectroscope; a small direct-vision one being used, as it was found that the changes in the spectrum were less distinctly seen with a spectroscope of greater dispersive power.

The absorption-spectrum of the cold glass is represented in fig. 1, which is drawn to an arbitrary scale, and not to one of wave-lengths. On gradually heating the glass, the absorption between the two first dark bands, those in the red and yellow, diminishes. The band in the red moves towards the least refrangible end of the spectrum, whilst those in the yellow and green retain their position, but become less distinct. Fig. 2 represents the spectrum of the hot glass.

As the glass is heated the intensity of its colour decreases; as it cools it recovers its original colour, and the absorption-spectrum changes back into that represented by fig. 1.

Fig. 1.

Fig. 2.



The position of the bands was measured, and the table gives the results, reduced to a scale of wave-lengths.

		Cold Glass.	Hot Glass.	Dr. Russell's Values.
Band I.		700	712	665
		636	655	632
Band II.		608	608	605
		580	583	588
Band III.		565	558	555
		indistinct	indistinct	

Dr. W. J. Russell, in a paper on the "Absorption Spectra of Cobalt Salts" (Proc. Roy. Soc. xxxii. p. 258) gives a map of the spectrum of cobalt glass drawn to a scale of wave-lengths. The position of the bands, as shown in Dr. Russell's map, is given in the third column of the above table.

The agreement between the values obtained with the cobalt glass when cold and those from Dr. Russell's map, except for the least refrangible edge of the first band, are as close as, perhaps, could well be expected, considering the small dispersion of the spectroscope used for the measurements, and the difficulty of determining the exact position of an absorption band. There is considerable difference with regard to the position of the least refrangible edge of band 1; that the position assigned to it for the particular sample of glass used in these experiments is, at least, approximately correct, is shown by the fact that the red lithium line of wave-length

670 lies within the band, both when the glass is cold and when it is hot.

These observations and those of Feussner show that the absorption-spectra of some substances vary with the temperature, as indeed might have been anticipated from the behaviour of the blowpipe-beads already referred to. In the case of solutions this may be due to the formation of different hydrates, or to the partial dissociation of the substances; but in the case of a solid substance, like cobalt glass, an actual change in the chemical constitution of the glass at a temperature considerably below its fusing-point does not seem very probable, although the well-known effects to light in causing glass which has been decolorized with manganese dioxide to become purple seems to show that such a change is not impossible.

XII. *Further Contributions to Dynamometry, or the Measurement of Power.* *By* T. H. Blakesley, *M.A., M. Inst. C.E.**

Now that the advantage of using the split dynamometer to measure power, as first proposed by me more than half a decade since, seems to be at length receiving some attention through the generalization of the method when applied to transformers, proved independently by Prof. W. E. Ayrton and myself †, it may be well to point out clearly the principles upon which such an instrument must be inserted into an electrical system to effect the measurement of a physical quantity, and the nature of the quantities which admit of such measurement.

In the first place, an exact idea must be formed of the nature of the physical quantity indicated by the reading of a dynamometer, or the angle through which the torsion-head is turned to bring the coils into a standard relative position, which is usually, but not necessarily, one in which the coils are at right angles one to the other. That position has the advantage of introducing no mutual induction in the instrument itself.

* Read February 27, 1891.

† To Professor Ayrton and Mr. Taylor belongs undoubtedly the credit of priority in this generalization. Professor Ayrton informed me of the *fact*, but left me to discover the *proof*.—T. H. B.

Expressed mathematically, the reading measures the quantity

$$\frac{1}{T}\int_0^T C_1 C_2 dt;$$

where C_1 and C_2 are the currents at a moment through the two coils, those currents being periodic or constant (one may be constant, the other periodic), and T being an interval of time at least equal to the least common measure of the periods, and so small that the index is not able to move appreciably in the interval T. The larger the moment of inertia of the moving coil, the greater the limit which may be allowed to T.

In the year 1885, when I first suggested sending different currents through the two coils of such an instrument, I called a reading taken under such circumstances the "force-reading," to distinguish it from an ordinary dynamometer-reading in the usual case of the currents being identical in the two coils. That name was suggested by the fact that $(\text{current})^2$ has for its structural formula in the electromagnetic system the same dimensions as force, omitting the dimension of permeability. This fact is shown in Sir W. Thomson's so-called current-balances, where $(\text{current})^2$ is made to produce equilibrium with a force.

But $(\text{current})^2$ has another more important meaning. When multiplied by resistance, it means power, and therefore by itself it means power per unit of resistance; and this is its true meaning independently of permeability. The dynamometer-reading is the *mean power* per unit of resistance.

If, therefore, we know the proper resistance to multiply the dynamometer-reading by, we shall be in possession of the value of the power; and it follows that appropriate dynamometer-readings must be of extreme value in measuring power.

It will thus be seen that if the physical quantity Z can be expressed for its momentary value in terms quadratic in the instantaneous currents, these terms will point out to us the appropriate places for dynamometers whose readings, being filled in in the places of those quadratic expressions, will give us the mean value of (Z). To make this perfectly clear :—

Suppose

$$Z = A \,.\, c_1^2 + Bc_1c_2 + Cc_2^2$$

at any instant; then the mean value of Z will be

$$A \,.\, {}_1D_1 + B\, {}_1D_2 + C\, {}_2D_2,$$

where ${}_1D_2$ is the reading of a dynamometer one of whose coils carries c_1 and the other c_2.

If Z is $\overline{\text{power}}$, A, B, C are of the order resistance. If Z is $\overline{\text{E.M.F.}^2}$, A, B, C are of the order (resistance)². It is necessary that A, B, C should not be functions of the time. Hence power and $\overline{\text{E.M.F.}}^2$, the latter being merely power per unit of conductivity, are very appropriate quantities for the method.

To take the simple case of two machines working in parallel into a third inductionless circuit. The equations are

$$e_1 - f = r_1 c_1,$$

$$e_2 - f = r_2 c_2,$$

$$f = r_3 c_3,$$

and

$$c_1 + c_2 = c_3 \,;$$

where e_1 and e_2 are the total E.M.F.s of the generators in the two loops 1 and 2 (including all induction); c_1, c_2, and c_3 are the three instantaneous currents, the two former positive towards the same point of junction, the latter positive towards the other, so that $c_1 + c_2 = c_3$ always; and f is the potential difference at the points where the circuits join.

Then, since

$$e_1 c_1 = r_1 c_1^2 + r_3 c_1 c_3 \text{ or } \overline{r_1 + r_3} c_1^2 + r_3 c_1 c_2,$$

$$e_2 c_2 = r_2 c_2^2 + r_3 c_2 c_3 \text{ or } \overline{r_2 + r_3} c_2^2 + r_3 c_1 c_2,$$

we have power of 1st generator

$$= r_1\, {}_1D_1 + r_3\, {}_1D_3, \text{ or } \overline{r_1 + r_3}\, {}_1D_1 + r_3\, {}_1D_2 \,;$$

power of 2nd generator

$$= r_2\, {}_2D_2 + r_3\, {}_2D_3, \text{ or } \overline{r_2 + r_3}\, {}_2D_2 + r_3\, {}_1D_2,$$

where D refers to a dynamometer-reading.

Here we appear to require four dynamometers; but the expression for the instantaneous power may be written in the 2nd form given, which necessitates only three dynamometers. Either generator here becomes a motor if the second term as given above has changed sign, and is of greater numerical value than the first term, which is necessarily positive.

The expressions for determining the mean $\overline{\text{E.M.F.}}^2$ of the machines are:—

$$\left.\begin{aligned}(\text{mean } e_1^2) &= r_1^2\,{}_1D_1 + r_3^2\,{}_3D_3 + 2r_1r_3\,{}_1D_3,\\ (\text{mean } e_2^2) &= r_2^2\,{}_2D_2 + r_3^2\,{}_3D_3 + 2r_2r_3\,{}_2D_3,\end{aligned}\right\}\text{ taking five dynamometer-readings.}$$

But this can be simplified, as in the formulæ for the powers, thus :—

$$e_1 = r_1c_1 + r_3c_3 = \overline{r_1 + r_3}c_1 + r_3c_2,$$

$$e_2 = r_2c_2 + r_3c_3 = \overline{r_2 + r_3}c_2 + r_3c_2\,;$$

$$\therefore\ e_1^2 = \overline{r_1 + r_3}^2\,{}_1D_1 + r_3^2\,{}_2D_2 + 2\cdot\overline{r_1 + r_3}\cdot r_3\,{}_1D_2,$$

$$e_2^2 = \overline{r_2 + r_3}^2\,{}_2D_2 + r_3^2\,{}_1D_1 + 2\cdot\overline{r_2 + r_3}\cdot r_3\,{}_1D_2\,;$$

in which expressions there are only three dynamometer-readings, and these the same three as for giving the two powers.

It is clear that $-r_3c_3c_2$ is the power doing work upon the second circuit; for it is equal to $-fc_2$ at any moment; $\therefore\ -r_3\,{}_2D_3$ is the mean power expended in the second circuit.

This is quite independent of the nature of the apparatus in the second circuit, which may contain any or all of the following :—

A perfect or absorbent condenser,
An electromagnet,
A decomposing-cell,
A vacuum-tube,
A motor-circuit,
A transformer-circuit,
A generating-circuit,
A welding-machine,
A tuning-fork, or other make and break.

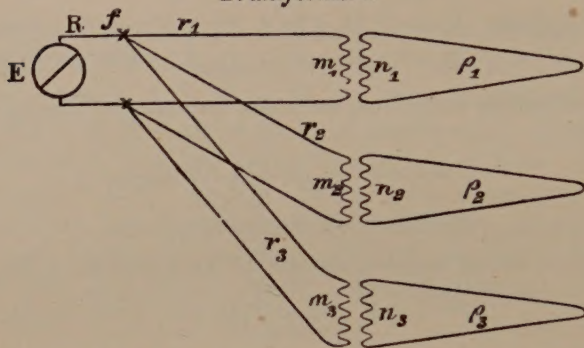
Should the apparatus render it undesirable to have the current c_2 passed through the dynamometer, we may write

$$-r_3c_3c_2 = -r_3c_3(c_3 - c_1) = r_3c_1c_3 - r_3c_3^2,$$

or $$\text{Mean Power} = r_3\{{}_1D_3 - {}_3D_3\}.$$

It was by this means that I suggested to Mr. Swinburne he might measure the dielectric hysteresis of his condensers. It would only take two dynamometers, as is seen.

The Case of a Machine playing into a Number of Parallel Transformers.



Let E be the E.M.F. of the machine (including all induction);
R its resistance ;
f the P.D. where the circuits become parallel ;
r the resistance of the primary from this point ;
m the number of turns in the primary coil;
n the number of turns in the secondary coil ;
ρ the resistance of the secondary coil.
C is the current through the generator ;
c is the current in the primary;
γ is the current in the secondary;
N the number of magnetic lines in the core.

Numerical subscripts must be applied where required for the various circuits.

Then

$$\left.\begin{array}{l} E-f=CR, \\ \left.\begin{array}{l} f-m\dfrac{dN}{dt}=cr, \\ n\dfrac{dN}{dt}=\gamma\rho, \end{array}\right\} \therefore f=cr+\dfrac{m}{n}\gamma\rho\,; \end{array}\right\} \therefore \quad E=CR+cr+\frac{m}{n}\gamma\rho;$$

multiplying f by c, $$fc=c^2r+\rho\frac{m}{n}\gamma c,$$

which gives the power in each transformer circuit, and indicates the appropriate positions of the two instruments, viz. one with both coils in the primary, and one split between the primary and the secondary.

$\rho\dfrac{m}{n}\gamma c$ is the power heating transformer and secondary,

$\rho\gamma^2$ is the power heating the secondary;

$\therefore\ \rho\frac{m}{n}\gamma c-\rho\gamma^2$ is the power heating the core, being the result of hysteresis and Foucault currents.

Multiply E by C, and we have the total power, which is

$$\mathrm{R}\,.\,\mathrm{C}^2+r\mathrm{C}c+\frac{m}{n}\rho\,.\,\mathrm{C}\gamma.$$

Thus the whole power consumed in such a system may be measured by means of three dynamometers judiciously applied to the main current and those in *a single* branch.

This, of course, can be applied to measure the power at work in a system of parallel transformers by means of a home transformer, whose resistances may be kept fixed or at least accurately determined.

It may also be used to measure the electric power of a *welding* transformer, into whose secondary it is inexpedient, if not impossible, to introduce any extra resistance.

In this case a very high power would be rendered measurable by increasing the resistances in the circuits of the parallel system. The power consumed in the parallel system being, as indicated, known, can be deducted from the total.

Transformers in Series.

Let E be the E.M.F. of the machine;

r_1 be the resistance of the 1st primary;

r_2 " " 1st secondary and 2nd primary;

r_3 " " 2nd " 3rd "

N_1 be the magnetic lines in 1st core;

N_2 " " 2nd "

$\left.\begin{matrix} m_1 \\ n_1 \end{matrix}\right\}$ = turns in $\left\{\begin{matrix} \text{primary} \\ \text{secondary} \end{matrix}\right\}$ of 1st transformer;

$\left.\begin{matrix} m_2 \\ n_2 \end{matrix}\right\}$ = " " 2nd "

c_1 = current in 1st section;

c_2 = " 2nd "

Then

$$\mathrm{E}-m_1\frac{d\mathrm{N}_1}{dt}=r_1c_1;$$

$$n_1\frac{d\mathrm{N}_1}{dt}-m_2\frac{d\mathrm{N}_2}{dt}=r_2c_2,$$

$$n\,\frac{d\mathrm{N}_2}{dt}-m_3\,\frac{d\mathrm{N}_3}{dt}=r_3c_3,$$

&c., &c.

$$\therefore\ \mathrm{E}=r_1c_1+\frac{m_1}{n_1}\,r_2c_2+\frac{m_1m_2}{n_1n_2}\,r_3c_3+\frac{m_1m_2m_3}{n_1n_2n_3}\,r_4c_4+\text{\&c.}$$

The series has $\overline{q+1}$ terms when there are q transformers. The square of this expression will have every term quadratic in current, and be thus amenable to dynamometer treatment.

The value for one transformer has already been given by me.

If we multiply through by c_1, we have the total power c_1E given in suitable terms.

For one transformer,

$$c_1E = r_1c_1^2 \quad + \frac{m}{n} r_2c_2c_1;$$

or power

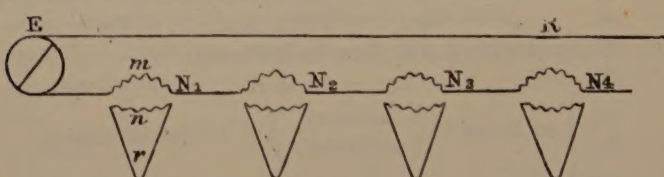
$$= r_{1\,1}D_1 + \frac{m}{n} r_{2\,1}D_2,$$

the first term heating the primary; $r_{2\,2}D_2$ is the power heating the secondary.

Therefore the power involved in warming the core by the magnetic changes is

$$\frac{m}{n} r_{2\,1}D_2 - r_{2\,2}D_2 \quad = r_2\left\{\frac{m}{n}\,{}_1D_2 - {}_2D_2\right\}.$$

The Case of Transformers with Primaries in Series.



E is the E.M.F. of the generator ;
N the number of magnetic lines in the core ;
m the number of turns in the primary of a transformer;
n the number of turns in the secondary of a transformer ;
C the current in the primary ;
R its resistance ;
c the current in the secondary ;
r its resistance.

Numerical subscripts being added where required.

Then

$$E - m_1 \frac{dN_1}{dt} - m_2 \frac{dN_2}{dt} - m_3 \frac{dN_3}{dt} - \&c. = CR.$$

$$n_1 \frac{dN_1}{dt} = c_1 r_1 ;$$

$$n_2 \frac{dN_2}{dt} = c_2 r_2 ;$$

$$n_3 \frac{dN_3}{dt} = c_3 r_3 ;$$

$$\&c. \qquad \&c.$$

$$\therefore \; E = CR + \frac{m_1}{n_1} c_1 r_1 + \frac{m_2}{n_2} c_2 r_2 + \frac{m_3}{n_3} c_3 r_3 + \&c.$$

The square of this will be quadratic in c, and the terms will indicate the proper places for dynamometers. The total power is

$$EC = C^2 R + r_1 \frac{m_1}{n_1} Cc_1 + r_2 \frac{m_2}{n_2} Cc_2 + \&c.$$

The first term heats the primary, and each succeeding term indicates the power employed in heating a secondary and a core corresponding with it. Both in this case and in the case of parallel transformers it appears that the power heating the core and its secondary is indicated by one dynamometer-reading alone, one coil being in the primary and the other in the secondary; the reading requiring multiplication by the ratio of the coil-turns in the primary to those in the secondary, and by the secondary resistance ; *i. e.* this power

$$= r_2 \, . \frac{m}{n} \, {}_1D_2.$$

Does not this indicate the direction which efforts should take to effect a really fair mode of measuring Electrical Energy supplied ?

In some cases a single instrument might be used, even if the formula indicated two terms, to obtain the required measurement. Suppose, for instance, that the formula had two terms $(c_1^2 - c_1 c_2)$. This may be written $c_1(c_1 - c_2)$; and it is clear that if we had two fixed similar coils both in one plane, and made to carry c_1 and c_2 *reversed*, respectively, and if the movable coil were made to carry c_1, then the indications of the instrument would give the required measurement. It might be

possible to multiply such coils, and vary their turns and position so as to meet any case, if desirable. The method is merely indicated here.

From what I have said, it will not surprise those who have followed me that in questions of power I recommend that quotations should be made of mean $\overline{\text{current}}^2$, as indicated by dynamometers, and that attempts at giving mean current in amperes, with reversing currents, should be given up. What a quantity of pains has been taken to make voltmeters give indications proportional to volts! The merit of a difference of potential is as its square, and that of cells as the square of their E.M.F. Give me twice the E.M.F., you quadruple my power of doing work by its means. To reduce readings to give the square root of mean square is doubly wrong. It is a ridiculous attempt to reach a useless quantity, and, further, gives one the trouble of squaring back again.

XIII. *Proof of the Generality of certain Formulæ published for a Special Case by* Mr. Blakesley. *By* Prof. W. E. Ayrton, *F.R.S., and* J. F. Taylor*.

I.

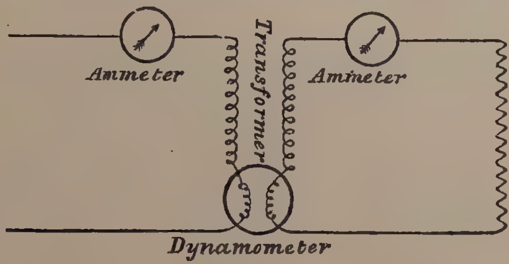
In May 1888 Mr. Blakesley described before this Society† a very interesting method of testing the power given to the primary coil of a transformer based on the employment of three dynamometers. The method, however, really only requires two alternate current ammeters and one dynamometer with two coils. The ammeters are placed respectively in the primary and secondary circuits of the transformer, while the two coils of the dynamometer are electrically separated from one another, one being placed in the primary and the other in the secondary circuit of the transformer, as shown in the figure.

Mr. Blakesley in his paper assumed that the three instruments were not graduated directly; that is to say, that constants had to be employed in each case to reduce the arbitrary

* Read February 27, 1891.

† Proc. Phys. Soc. ix. p. 286; Phil. Mag. [5] xxvi. p. 34.

scale-readings to absolute measure. We will, on the contrary, suppose the instruments to be graduated as they ought to be,



so that if D_p, D_s, and D_{ps} be the readings of the three instruments, they are equal respectively to the square root of the mean square of the primary current, the square root of the mean square of the secondary current, and the mean product of the two currents.

With this definition Mr. Blakesley proved geometrically that the watts given to the primary coil of the transformer were equal to

$$pD_p^2 + \frac{P}{S} sD_{ps};$$

where p and s are the resistances, in ohms, of the primary coil and the whole secondary circuit respectively, and P and S are the numbers of windings in the primary and secondary coils of the transformer.

The formula is a simple one, and the values of the expressions in it are fairly easy to obtain experimentally. The proof of the formula, however, as given by Mr. Blakesley, was based on the following assumptions :—

1. The variations of the primary and secondary currents are harmonic.

2. The variation of the magnetism of the core is harmonic.

3. The magnetic stresses produced in the iron core by the currents in the primary and secondary coils are directly proportional to the ampere-turns in these two coils.

4. Each turn in each coil embraces at any moment the same number of lines of force.

5. The secondary circuit outside the transformer is non-inductive.

II.

In the spring of last year, 1890, Mr. Wightman, one of the third-year students of the Central Institution, showed that an analytical method for measuring the efficiency of a transformer, which had been described in one of the lectures at the college, could by a slight transformation be employed to prove the generality of Mr. Blakesley's formula given above.

The proof is quite simple, and shows that the formula in question is true whatever function the currents or the magnetism be of the time, and whatever amount of hysteresis or magnetic lag may exist. In fact the proof is independent of Mr. Blakesley's assumptions Nos. 1, 2, and 3, mentioned above.

We have delayed the publication of this proof until we had used the method for a lengthy series of experiments on a transformer kindly lent us by Mr. Mordey, and which has occupied the students for many months. But, in order that others like ourselves should be able to use Mr. Blakesley's formula with confidence, and without having any longer the fear that if the sine law were not true, or if much hysteresis existed, their calculations made by means of this formula from experimental results might be very wrong, we communicated nearly a year ago to Mr. Blakesley and to others interested in the matter the fact that we had proved mathematically that the two ammeters and one dynamometer method of measuring power was generally true.

Let V_p be the P.D. in volts at the terminals of the primary-coil at any moment t.

Let A_p be the primary current, in amperes, at the same moment.

Let A_s be the secondary current, in amperes, at the same moment.

Let n be the number of lines of force passing through one convolution at that moment.

Then

$$V_p = pA_p + \frac{1}{10^8} P \frac{dn}{dt},$$

$$sA_s = \frac{1}{10^8} S \frac{dn}{dt};$$

$$\therefore \quad V_p = pA_p + \frac{P}{S} sA_s.$$

$$\therefore A_p V_p = pA_p^2 + \frac{P}{S} sA_p A_s.$$

If T be the time of one complete cycle,

$$\frac{1}{T}\int_0^T A_p V_p dt = \frac{p}{T}\int_0^T A_p^2 dt + \frac{P}{S}\frac{s}{T}\int_0^T A_p A_s dt.$$

But the expression on the left-hand side is the mean watts given to the primary coil, and the expression on the right-hand side is simply

$$pD_p^2 + \frac{P}{S} sD_{ps};$$

D_p, as already explained, being the reading of the alternate current ammeter in the primary circuit, and D_{ps} the reading of the dynamometer having one coil in the primary and the other coil in the secondary circuit.

The following is another general proof of the same formula :—

The watts given to the primary coil of a transformer are spent partly in heating the primary coil, and partly in doing work against the back electromotive force set up by the varying magnetism of the core.

The watts spent in heating the primary coil are of course pD_p^2; while the watts spent in doing work against the back electromotive force are at any moment $A_p \times$ the back E.M.F.

The E.M.F. generated in the secondary circuit by the same variation of magnetism of iron is at any moment sA_s; therefore the back E.M.F. in the primary coil at the same moment must be $\frac{P}{S} sA_s$.

Consequently the total watts given to the primary coil are

$$pD_p^2 + \frac{P}{S}\frac{s}{T}\int_0^T A_p A_s dt;$$

$$pD_p^2 + \frac{P}{S} s\, D_{ps}, \text{ as before.}$$

III.

This expression, as Mr. Blakesley points out, may be written as follows :—

$$pD_p^2 + sD_s^2 + s\left\{\frac{P}{S}D_{ps} - D_s^2\right\};$$

and since $pD_p^2 + sD_s^2$ are the watts employed in heating the primary coil and the whole of the secondary circuit, it follows that

$$s\left\{\frac{P}{S}D_{ps} - D_s^2\right\}$$

are the watts employed in heating the core on account of hysteresis or magnetic lag ; a result now proved true independently of all assumptions as regards the sine law, or the magnetic stress being directly proportional to the ampere-turns, &c.

IV.

Since the portion of the secondary circuit outside the transformer is non-inductive and has a resistance s' say, the watts developed in it are of course $s'D_s^2$. Consequently the efficiency of the transformer is

$$\frac{s'D_s^2}{pD_p^2 + \frac{P}{S}sD_{ps}}.$$

V.

Using the various assumptions already referred to, Mr. Blakesley arrived, by means of a geometrical proof, at a formula for measuring the mean square of the P.D. at the terminals of the primary coil by means of the two ammeters and the dynamometer.

The following *general* proof of this formula is very much simpler than the proof for only a special case which Mr. Blakesley gives, and furnishes a very good example of the fact that sometimes it is more easy to give an analytical proof which is true independently of any assumptions about the harmonic law, &c. than to give a geometrical proof which is only true when these suppositions hold.

$$V_p = pA_p + \text{the back E.M.F. at the moment}$$
$$= pA_p + \frac{P}{S} sA_s ;$$
$$\therefore \frac{1}{T}\int_0^T V_p^2 dt = \frac{p^2}{T}\int_0^T A_p^2 dt + \frac{P^2 s^2}{S^2 T}\int_0^T A_s^2 dt + 2\frac{P}{S}\frac{ps}{T}\int_0^T A_p A_s dt ;$$

that is, the mean square of the P.D. at the terminals of the primary coil equals

$$pD_p^2 + \frac{P^2}{S^2} s^2 D_s^2 + 2\frac{P}{S} ps D_{ds},$$

which is the formula given by Mr. Blakesley.

From the preceding it follows that Mr. Blakesley's expressions for the watts given to the primary coil of a transformer, for the efficiency of the transformer, and for the mean square of the P.D. at the terminals of the primary coil, are true irrespectively of any assumptions as to the functions the E.M.F.s, the currents, or the magnetic flux are of the time as well as of any assumptions as to the presence or absence of hysteresis or magnetic lag.

This being the case, the application of this two ammeters and dynamometer method for measuring power in other cases than those already treated of is worthy of careful consideration.

XIV. *On the Variation of Surface-Tension with Temperature.*

By Prof. A. L. Selby, *M.A., University College, Cardiff* *.

Mendelejeff speaks of an ideal liquid as characterized by two conditions :—

(1) $V_t = V_0/(1-kt)$, V_t being the specific volume at $t°$ C.

(2) $T_t = T_0(1-at)$, T_t being the surface-tension at $t°$ C.

I believe that the following proof shows that all liquids satisfy Mendelejeff's second condition.

Let unit mass of liquid have a constant volume, but variable surface S, and temperature t.

* Read March 20, 1891.

In a small change of the variables, the heat absorbed is

$$dH = kdt + ldS,$$

k being the specific heat at constant volume, l the latent heat of extension.

The external work done on the film is

$$dW = TdS.$$

Therefore the gain of intrinsic energy is

$$dH + dW = kdt + (l+T)dS.$$

This is a perfect differential.

Therefore

$$\frac{dk}{dS} = \frac{d(l+T)}{dt}.$$

Also $\frac{dH}{t}$ is a perfect differential.

Therefore

$$\frac{d}{dS}\frac{k}{t} = \frac{d}{dt}\frac{l}{t}.$$

Therefore

$$\frac{1}{t}\frac{dk}{dS} = \frac{1}{t}\frac{dl}{dt} - \frac{l}{t^2}.$$

Therefore

$$\frac{dT}{dt} = -\frac{l}{t}.$$

And

$$\frac{d^2T}{dt^2} = -\frac{1}{t}\frac{dl}{dt} + \frac{l}{t^2} = \frac{1}{t}\frac{dk}{dS}.$$

Now k does not depend on the surface unless the film is very thin.

Therefore

$$\frac{d^2T}{dt^2} = 0.$$

And

$$T = c - bt,$$

where c and b may be functions of the specific volume.

That b does not depend on the specific volume may be shown as follows.

Let the liquid be maintained at constant temperature, and have a volume v and surface S; and let L be the latent heat of dilatation at constant surface.

Put the liquid through a cycle consisting of two isometrics v, $v+dv$, and two lines of constant surface S, S+dS.

Since the cycle is reversible and the temperature is constant, the heat absorbed is zero.

Therefore

$$\frac{dl}{dv}-\frac{d\mathrm{L}}{d\mathrm{S}}=0.$$

Therefore l is independent of v, and so is b since $l=bt$.

Thus the latent heat of extension is proportional to the absolute temperature. This agrees with a hypothesis of Clausius (Phil. Mag. 1862, vol. xxiv.).

It has been shown that T can be expressed in the form

$$f(v)-bt.$$

We shall show that it can also be written $\phi(p)-bt$.

For let the pressure of the liquid remain constant while the surface, volume, and temperature vary.

Then

$$d\mathrm{H}=\mathrm{K}dt+ld\mathrm{S},$$

K being the specific heat at constant pressure and l having the same meaning as before, for the latent heat of extension at constant temperature and volume is also the latent heat of extension at constant pressure (and temperature).

The external work done on the liquid is

$$d\mathrm{W}=\mathrm{T}d\mathrm{S}-p\frac{dv}{dt}dt,$$

p being regarded as constant in forming $\frac{dv}{dt}$.

Therefore,

$$d\mathrm{H}+d\mathrm{W}=\left(\mathrm{K}-p\frac{dv}{dt}\right)dt+(l-\mathrm{T})d\mathrm{S}.$$

Since this is a perfect differential,

$\frac{d}{dt}(l+\mathrm{T})=\frac{d}{d\mathrm{S}}\left(\mathrm{K}-p\frac{dv}{dt}\right)=0$, except for a very thin film.

Now $\quad l=bt.$

Therefore $\quad \mathrm{T}=\phi(p)-bt.$

But the two expressions $\phi(p)-bt$ and $f(v)-bt$ can only be

identical at all temperatures if $f(v)$ and $\phi(p)$ are both equal to a constant c.

Therefore

$$T = c - bt,$$

where c and b are constant.

It appears then that the surface-tension of a liquid is independent of the pressure and depends only on the temperature, unless the film is very thin.

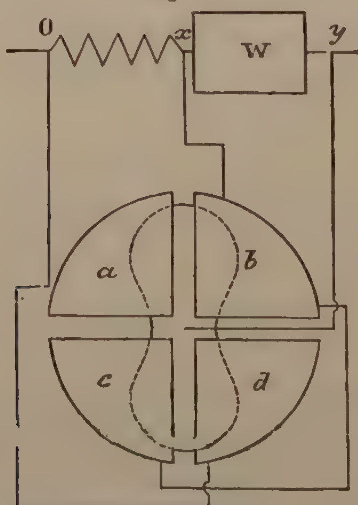
The critical temperature is c/b, and can be found by determining the surface-tension at two very different temperatures.

XV. *The Electrometer as a Wattmeter.*

By J. Swinburne *.

In 1881, when M. Joubert published his experiments on a Siemens machine, in the course of which he had used a Thomson or Mascart electrometer as a voltmeter, Professors Ayrton and Fitzgerald simultaneously proposed to use the quadrant electrometer as a wattmeter.

Fig. 1.



The ordinary method of arranging the instrument is shown in fig. 1. The resistance is wound so as to be non-inductive ;

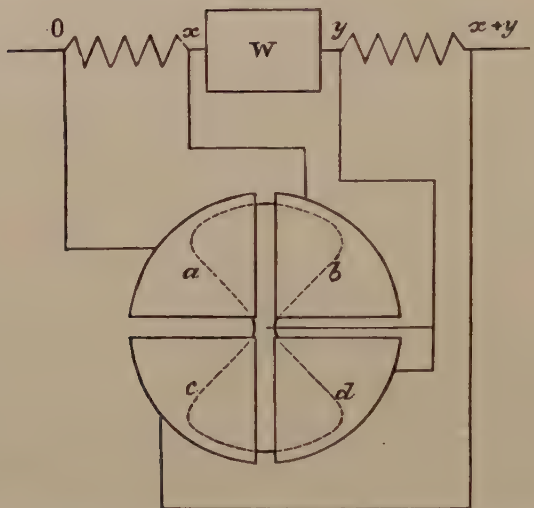
* Read March 6, 1891.

and the power to be measured is spent in the apparatus marked W. If the fall of potential between x and y is very great in comparison with that over the resistance, the instrument reads like a charged electrometer, and it may be taken to read in watts. In practice such conditions do not occur; for if the resistance is made low the instrument is not sensitive enough, and if it is made high the electrometer no longer reads in watts. If the instantaneous pressures are 0, x, and y, the force exercised by the first quadrant a in the positive direction is $k\ y^2$, where k is the constant of the instrument. This constant may be omitted, and the force denoted by y^2. The force exercised by b is $-(y-x)^2$; by $c, -(y-x)^2$; and by d, y^2. The total force is thus $2\ (2y-x)\ x$. The needle contact may now be moved from y to x and another reading taken. This is of course $2\ x^2$. Subtracting this from the first reading, we get $4\ (y-x)\ x$, which is the power taken by W. This is the arrangement that was adopted by Dr. Hopkinson in his measurements of the Gaulard and Gibbs transformer in 1884. During the first reading the instrument is really two idiostatic voltmeters, one being in shunt to both the resistance and W, and the other in shunt to W alone. During the second reading it is a voltmeter in shunt to the resistance alone.

Professor Ayrton arranges the instrument so that the quadrants are in shunt to W, and the needle is first connected to x and a reading taken, and the needle is then connected to 0 and a second reading taken. During the first reading, the instrument is a voltmeter in shunt to W, and during the second it is two voltmeters, one in shunt to the resistance and one in shunt to the whole circuit. The difference again gives the power spent in W. Mr. Smith uses a discharge-key for making a change quickly. The discharge-key can be used in either arrangement, but the first is more accurate. For instance, suppose $y-x$, the pressure on W, is 2000 volts, and x, the pressure on the resistance, 20, and suppose the instrument one per cent. low at one reading. The first reading by the second method is 8,000,000, the second 8,160,000. Suppose the latter is read one per cent. low, viz. 8,078,400, the resulting determination is 78,400 instead of 160,000; that is to say, the power is more than 100 per cent. greater than that given by the instrument. In the arrangement used by Dr. Hopkinson,

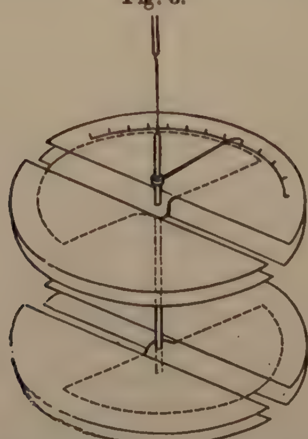
however, the readings are 160,800 and 800, so a misreading of one per cent. in the first makes one per cent. error only, and in the second makes no sensible difference.

Fig. 2.



The quadrant electrometer may, however, be arranged so as to read power directly without any change of connexions. This is shown in fig. 2. A second resistance equal to the

Fig. 3.



first is put on the other side of W, and the quadrants are connected up as shown. The first quadrant *a* then has a force

y^2, the second $b, -(y-x)^2$. So far the instrument is like that shown in fig. 1, but with only half its quadrant utilized. Quadrant c has a force $-x^2$, and d is inactive; so that quadrant c makes the correction for which the second reading was necessary in fig. 1, so the instrument can be graduated in watts.

There may be some slight error due to the whole needle being drawn into the quadrants a and b, and out of c and d. This is obviated by making the needle of such a shape that there is no appreciable end pull, as shown in fig. 2.

Fig. 3 shows a form for a direct-reading instrument with a pointer or index. Instead of quadrants it has half-disks, like the Blondlot and Curie electrometer, but, unlike it, this form has four pairs of half-disks. The Blondlot and Curie form also reads power directly, but the needle is made in two insulated portions, and needs two metallic connexions, and this gives rise to mechanical troubles and loss of sensitiveness. The instrument shown in fig. 3 has the needle all in one piece, and the disks can be so far apart that errors from variations of height of the needle due to variations of the length of the fibre do not become serious. A long suspension of phosphor-bronze wire is, however, preferable for most purposes.

XVI. *Alternating and Experimental Influence-Machine.* *By* Mr. James Wimshurst, *Member of Council**.

I have pleasure in bringing to your notice a new form of influence-machine which is self-exciting, notwithstanding that when at work its electrical charges alternate during each revolution.

In order that you may readily follow the action of the machine when at work I will first describe its construction in all its details.

It consists of a base or frame, from the sides of which rise the standards to carry the spindle and boss for the rotating disk, suitable driving gear being fixed thereto.

In the same plane as the rotating disk is fixed a square wooden frame having the necessary holes, plugs, and clamps, by means of which the inductor-plates are held in position.

* Read April 17, 1891.

The rotating disks are cut from ordinary window glass, and are coated with shellac; they are 16 inches in diameter; one of them has no metal upon it, the next has four medium-sized tin-foil sectors upon each of its sides, the last of this series has 16 sectors upon each side; other disks have from 2 to 4 sectors of large size upon them; another disk has four large sectors upon each side, so placed that the sectors upon one side cover those upon the other side; another has four very narrow sectors upon each side, and another has 16 sectors all upon the one side.

The inductor-plates are squares of glass measuring $9\frac{3}{4}$ inches; one corner of the plate is cut away to admit the spindle and the boss. They are coated with shellac, and upon one side of each of them is a tin-foil patch, and a suitable device for holding the rod and the brush.

Two of these inductor-plates are mounted at the diagonal corners, upon one side of the wooden frame, and two upon the other side of the frame; those at the front of the machine are at the lower right-hand corner and the upper left-hand corner; those at the back of the machine are at the upper right-hand corner and the lower left-hand corner: the rotating disk is therefore covered for one half of its surface upon both of its sides.

The brushes are made of fine brass wire, and the brush-holders are brass rods, bent to a form to admit of the brushes touching the rotating disk at a point opposite to the middle of the next following induction-plate; this arrangement supplies two brushes to each side of the disk, and the brushes when in place are situated 180° asunder.

The several parts of the machine are interchangeable, and by means of the varied combinations many experiments can be made; in fact its combinations include nearly every type of electrical influence-machine.

The prominent results obtained from it are:—(1) That glass disks which have no metal upon them are freely self-exciting. (2) That the freedom to self-excitement increases about proportionally to the number of sectors. (3) That the quantity of electricity decreases with the amount of metal upon the disk—whether the amount be in the greater number of sectors, or the increased size of sector (chiefly the latter).

The only tests as to when the alternations occur which I have been able to make were made by means of a sensitive arrangement of light paper disks, suspended by fine wire. When this apparatus is connected to one of the inductors, and the glass disk turned very slowly, the alternations are seen to occur with each $\frac{3}{4}$ revolution of the disk; when the disk is turned much faster, then the alternations occur too rapidly for the paper disks to respond, and they hang motionless and nearly together.

It is not possible to obtain any sensible charge in a Leyden jar, although the electricity may be clearly seen as a stream between the jar and the inductor.

Another series of combinations may be made by removing the two inductor-plates from the back of the machine and substituting an insulating arm extending across the disk, it having wire brushes at its ends, the brushes being connected metallically with terminal balls. When this combination is in use the charges no longer alternate unless the terminal balls are separated beyond the sparking distance. The plain glass disk without metal sectors, and also the disks having large sectors upon them, are no longer self-exciting. The glass disk having 16 sectors upon one side is not self-exciting when placed so that the sectors touch the brushes of the inductors, but when placed so that the sectors touch the brushes of the insulating arm it then becomes freely self-exciting. All disks having medium-sized sectors upon each side are freely self-exciting.

I have noted many of these results in tabular form.

I now feel uncertain as to whether I should end my paper at this stage, or whether I should extend it into the region of opinion. If I remain silent I am sure the cause of the electrical action will be dealt with by abler minds than my own. On the other hand, I feel equally sure that many will wish me to indicate a working hypothesis; therefore, and by way of suggestion, I will add what seems to me to be a reasonable explanation.

As to the initial charge, I think it may be accepted that all bodies behave as though they possessed a film of electricity over their surfaces, and that when two or more of these bodies are brought together this normal electrical condition is upset,

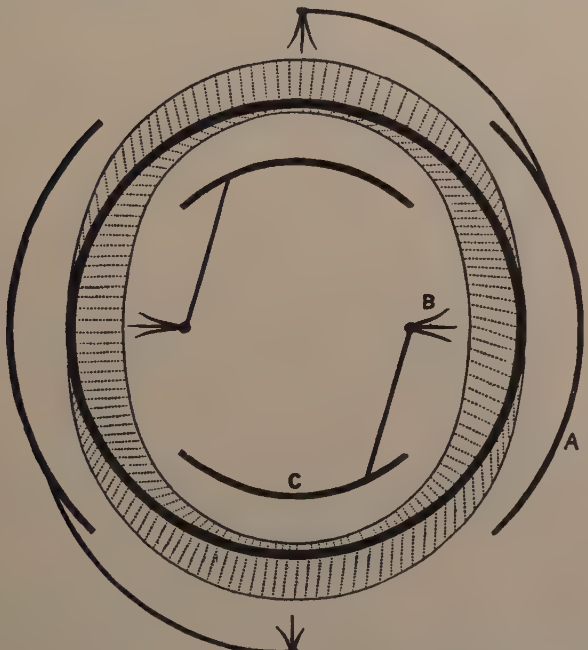
Table showing Results of Combinations.

Disks.	Four Large Inductors.	Four Small Inductors.	Two Inductors and Rod.
Plain glass and no metal.	Is self-exciting with about 4 revolutions. Alternations occur each $\frac{3}{4}$ revolution.	Is self-exciting but requires care. Alternations occur each $\frac{3}{4}$ revolution.	Is not self-exciting but requires little assistance. Constant current while not overtaxed; gives large current.
Two metal sectors each side.	Is self-exciting. Alternations occur after each two sectors pass brush.	Is self-exciting. Alternations occur each $\frac{3}{4}$ revolution.	Is self-exciting. Constant current.
Four metal sectors each side.	Is self-exciting. Alternations occur each $\frac{3}{4}$ revolution.	Is self-exciting. Alternations occur each $\frac{3}{4}$ revolution.	Is self-exciting. Constant, but gives less current.
Eight metal sectors each side.	Is more freely self-exciting. Alternations occur each $\frac{3}{4}$ revolution.	Is self-exciting. Alternations occur each $\frac{3}{4}$ revolution.	Is self-exciting. Constant current.
Sixteen metal sectors each side.	Is freely self-exciting with 3 revolutions. Alternations occur each $\frac{3}{4}$ revolution.	Is more freely self-exciting. Alternations occur each $\frac{3}{4}$ revolution.	Is freely self-exciting. Constant, but gives less current.
Sixteen sectors on one side.	Is self-exciting. Alternations occur each $\frac{3}{4}$ revolution.	Is self-exciting. Alternations occur each $\frac{3}{4}$ revolution.	Is self-exciting when sectors are next the rod. Is not self-exciting when sectors are next inductors. Constant in either case.

NOTE.—The principal features are:—1. That glass disks with no metal upon them are capable of self-excitement. 2. That the self-excitement increases about proportionally to the increase in the number of sectors. 3. That the quantity of electricity decreases about proportionally to size of sectors and also their number.

for the electricity which was upon their near surfaces is repelled to the outer or bounding surface—it is this excess charge upon the bounding surface, minute it may be, which constitutes the first charge.

In respect to the electrical action which takes place in this particular machine, I will endeavour by the help of the small diagram to explain.



Assuming that we have obtained an initial charge and that it is brought to the inductor marked A, then, neglecting the changes which take place between the two bounding surfaces, we obtain by induction an excess of electricity upon the far surface B of the rotating disk: this excess in its turn is conveyed by the brush and its holder to the next inductor C, which in its turn repeats the operation, and so on with each inductor, for the inductors are, as you see, situated alternately, the first upon one side, and the next upon the other side of the rotating disk. These excesses of electricity seated upon the rotating disk, opposite to the inductors, may be viewed as wave-crests, while the corresponding depressions are under

the surface of the inductor. All that is done by the machine is to produce this wave-action in the electrical coating or film; for there is no metallic connexion between inductor and inductor, nor between the machine and the earth: moreover, all the inductors are charged with electricity of one sign, although, probably, the potential in one inductor may be slightly different to that in the other.

The alternations are possibly caused by the repulsion between the electrical charges upon the disk and the inductor, and the consequent slipping of the electrical film upon the inductor in such manner as to produce a break in the phase of the wave in relation to the brushes.

XVII. *Some Points in Electrolysis. By* J. Swinburne*.

Let a cell be considered, with its external circuit closed through a resistance so high that in comparison the internal resistance is negligible. The cell discharges, and in passing from one pole to the other each coulomb does a certain quantity of work proportional to the difference of potential of the poles, E. This is really a mere definition: if difference of electric potential between two points is defined as what is measured by the work done on unit quantity of electricity passing from one to the other, the work done is numerically equal to E. The work done on the coulomb passing onward through the cell to regain its original position is equal to the work done when it passes through the external resistance. This work done on the coulomb may be supplied at the expense of chemical energy, or there may be local cooling somewhere. There may be chemical energy supplied at some points, and absorbed at others, and heat may disappear at some points, and may be evolved at others, but the algebraical sum comes out equal to E. Call the poles of the cell p and n, the positive pole being, for example, platinum and the negative such a metal as zinc, and suppose the electrolyte to be one homogeneous fluid. Part of the work done on the coulomb in passing from n to the electrolyte may

* Read March 20, 1891.

be supplied chemically and may be called E_{nc}, another part may be supplied by local cooling E_{nh}, c standing for chemical and h for heat. If there is really local heating this will be negative. There is no work done by the homogeneous electrolyte, and the work done on it, in overcoming its resistance, is assumed to be inappreciable. The work done on the coulomb passing from the electrolyte to the plate forming the pole p is similarly $E_{pc}+E_{ph}$. We have thus

$$E=E_{pc}+E_{nc}+E_{ph}+E_{nh}.$$

First assume that the chemical work done is not dependent on the temperature of the cell. Let it discharge one coulomb at temperature θ_1 and do work equal to E_{θ_1} joules; E_{θ_1} being the electromotive force of the cell at that temperature. Of this work $E_{nc\theta_1}+E_{pc\theta_1}$ is supplied by chemical changes, and $E_{nh\theta_1}+E_{ph\theta_1}$ by cooling of the cell. Let the cell now be heated to the temperature θ, and let it be treated as a secondary element, and charged with one coulomb. The work done upon the cell is then E_θ; so

$$E_\theta=E_{nc\theta}+E_{pc\theta}+E_{nh\theta}+E_{ph\theta}.$$

The chemical work $E_{nc\theta}$ and $E_{pc\theta}$ is common to both processes. If E_θ were equal to E_{θ_1}, the same work would be done at the two temperatures, or $E_{nh\theta_1}+E_{ph\theta_1}$ would be equal to $E_{nh\theta}+E_{ph\theta}$.

But on letting the temperature of the cell fall to θ_1 again $\frac{\theta-\theta_1}{\theta}(E_{nh\theta}+E_{ph\theta})$ is available for external work; so that we should have perpetual motion. E_θ must therefore be greater than E_{θ_1}, so that a margin is allowed for the available work. We thus, by simple reasoning, arrive at Helmholtz's equation,

$$E=E_{nc}+E_{pc}+\theta\frac{dE}{d\theta}.$$

The electromotive force needed to do the chemical work may also vary with the temperature. For instance, if the chemical changes involved in discharging are brought about in a calorimeter at different temperatures, different heats may be evolved. Suppose E_{nc} and E_{pc} are less at a high temperature, and suppose at the lower temperature θ_1 there are no Peltier effects at the plates, so that, for that temperature

at least, the cell obeys Sir William Thomson's law. If the cell could be charged at θ with a low electromotive force of $E_{c\theta}$, needed to bring the chemical changes about, and then discharged at θ, with a higher electromotive force, perpetual motion would be obtained. There must therefore be an absorption of electric energy at θ in addition to that needed to produce the chemical work. This must be liberated as heat, and some of this must be available; the cell must therefore not only have a Peltier effect at the higher temperature, but though the chemical work done at that temperature is less, the electromotive force must actually be greater. By taking a cell whose E_c varies with the temperature round the usual Carnot cycle, we get

$$\frac{dE}{d\theta}=\frac{E_h}{\theta}-\frac{dE_c}{\theta},$$

which shows that the temperature-coefficient of the cell is not affected by the variation of chemical work with the temperature. The term $-dE_c/\theta$ is the remains of the extra work that had to be allowed in compensating for the reduction of the chemical work in the example just taken. Cutting it out we get

$$E_h=\theta\frac{dE}{d\theta},$$

or

$$E=E_c+\theta\frac{dE}{d\theta},$$

which is Helmholtz's equation again. Writing the equation in full, with separate terms for the poles, we have

$$E_n+E_p=E_{nc}+E_{pc}+\theta\frac{dE_n}{d\theta}+\theta\frac{dE_p}{d\theta}.$$

The cell may be made up with the plates in separate vessels with a tube of electrolyte to connect them; and the vessels can be heated to different temperatures. As E_n, E_{nc}, and $\theta\frac{dE_n}{d\theta}$ depend on the temperature of their vessel, the last equation can be split up into two:—

$$E_n=E_{nc}+\theta\frac{dE_n}{d\theta}\text{ or }E_{nh}=\theta\frac{dE_n}{d\theta},$$

and

$$E_p = E_{pc} + \theta \frac{dE_p}{d\theta} \text{ or } E_{ph} = \theta \frac{dE_p}{d\theta};$$

and, as the temperature-coefficient of each contact can be found, the Peltier effect at each contact can be obtained separately.

The hypothetical cell discussed has only one fluid, and is reversible. It might be difficult to find such a cell. If two fluids are used, their junction can be arranged in a third intermediate vessel, and their Peltier effect, or difference of potential due to heat-formation, may be found as in the case of the plates.

Several workers, for instance M. Bouty and H. Gockel, have been investigating and measuring the temperature-coefficient or the Peltier effect directly.

Prof. J. Willard Gibbs has, of course, discussed the subject. In a letter to Dr. Lodge * he took a theoretical case in which this cell could be heated to the temperature of dissociation. He has written a second letter going more fully into the subject †.

This leads at once among the various conflicting views of electrolysis. One view, which seems tenable, is that electrolysis is always a case of double decomposition: that there is really a change. According to orthodox chemistry, if 2HCl is electrolysed H_2 and Cl_2 are obtained, and not 2H and 2Cl. The hydrogen and chlorine form combinations with themselves. Dissociation does not always split a compound in the same way as electrolysis, and the results of it are often "free atoms." It might be urged that in such a case as the electrolysis of NH_4Cl free atoms of NH_4 cannot exist, so NH_3 and HCl are produced by a sort of secondary action.

The cells discussed have been assumed to be reversible; and some question may arise as to what the chemical work means. For instance, in a Daniell cell are we to take such data as Zn,O=85,430 and Cu,O=37,160 ‡ from a convenient treatise on Thermochemistry, and to consider that the conver-

* B. A. Report, 1886, p. 388.

† B. A. Report, 1888, p. 343.

‡ J. Thomsen, *Thermochemische Untersuchungen*, iii. pp. 275 and 320.

sion into sulphate and other actions in the cell are secondary? It is to be regretted that writers on Thermochemistry seldom give their data so that their meaning is clear. Zn,O, for instance, often means that metallic zinc is burned in gaseous oxygen. Allowance must then be made for the physical states of the components before, and of the products after the combination.

Returning to the reversible cell, however, before discharging a coulomb there are certain quantities of certain substances in certain physical and chemical conditions. After the discharge certain chemical and physical changes have taken place. Suppose any one of these is a secondary, or non-adjuvant action. For instance, suppose in discharging a Daniell cell that the conversion of zinc into oxide is a primary action, and the conversion of zinc oxide into zinc sulphate secondary or non-adjuvant. Some heat will be evolved, and the change from oxide to sulphate will not appear as external electrical work. On charging again there is only enough electromotive force at that contact to cope with the attraction of zinc for oxygen and not with the greater attraction for SO_4. The result is that the cell is not reversible. Any secondary or non-adjuvant action in the cell thus means non-reversibility. We thus have to deal not merely with Zn,SO_4 or ZnO,SO_3aq, and so on, but in a reversible cell, like a Daniell, we have to consider the attraction of Zn for SO_4; the physical change of Zn from a solid to a liquid state, which is generally included in the "heat of combination," as given; the solution of the $ZnSO_4$; and perhaps such small matters as change of volume of the cell. The copper salts must be dealt with similarly. Even if the heat of solution were non-adjuvant the cell would not be reversible.

Since 1883 I believe I have been alone in holding that not only is lead sulphate formed on both plates of a secondary battery, as shown by Dr. Gladstone and Mr. Tribe in 1882, but that its formation is the cause of the action of the cell, and not a secondary reaction at all. That is to say, there is no intermediate formation of PbO. To find out E_c of a cell we must open a sort of book. On one side put the heat of formation of every compound formed on discharge, and on the other of every compound broken up on discharge, the balance,

including the physical changes, giving E_c. For instance, in a storage cell we credit the formation of $PbSO_4$ on the spongy plate, including, of course, the change of the SO_4 into the solid state, and on the peroxide plate we again credit $PbSO_4$. Two equivalents of H_2O are also credited. Two equivalents of $H_2SO_4 + Aq$ are debited, also two equivalents of H_2SO_4, and one equivalent of PbO_2, allowing for the O_2 becoming liquid.

It has been repeatedly urged that as the formation of $PbSO_4$ takes place on both plates it must cancel out. As in the case of a Daniell cell we have to deal with the difference between the heat of formation of two sulphates, it is assumed we must also deal with differences in a secondary battery. A little consideration will show, however, that the cases are different. In a discharging Daniell sulphate of zinc is formed and sulphate of copper is decomposed, so one goes on the credit side, the other on the debit. In a secondary battery sulphate is formed on both plates on discharge, and is therefore credited twice.

The doctrine of the Conservation of Energy also teaches us that electrolytic or nascent oxygen and hydrogen, which many chemists regard as such valuable reagents, do not exist. What is the evidence in favour of the existence of nascent hydrogen? If some such metal as magnesium, or sodium amalgam, is put in dilute acid, bubbles come off. Many other metals act similarly. If such a compound as persulphate of iron is put in, too, it is reduced to proto-sulphate. From frequently observing effervescence when reduction is effected, it is easy to assume the effervescence is the cause of the reduction. The theory is that the energy supplied first produces hydrogen. A powerful attraction has just been overcome, and has been satisfied neither by combination with more hydrogen nor otherwise. The hydrogen then seizes on the persalt of iron, and takes away some of the acid radical, forming free acid, which in its turn acts on the oxide of the metal. This action is secondary, and may be supposed to evolve heat. There is thus waste of heat, and the cell is irreversible. A better explanation would be, that the metal can dissolve if it either reduces the persalt or evolves hydrogen. The reduction of the persalt needs less energy, so that takes place. When there is no reducible salt

available, hydrogen is evolved; and as it has to be expanded into the gaseous form, a good deal of work has to be done on it. Evolution of hydrogen and reduction of the salt are thus alternate, not consecutive results. Similarly in an engine—the steam either works the engine or comes out at the safety-valve; it does not begin to lift the safety-valve, and then change its mind and work the engine in a nascent state. It must be remembered that the term oxidize has come to denote many other things than adding oxygen. For instance, adding any electronegative radical is called oxidizing. If sulphur had been as common as oxygen, no doubt we should always talk of sulphurizing. The use of the term "oxidize" has also led to the tacit assumption that in electrolysis the water is electrolysed and the other results are secondary actions of electrolytic oxygen and hydrogen. If there were such a thing as nascent hydrogen, putting a depolarizer, such as nitric acid, round the carbon plate of a Bunsen cell would not increase its electromotive force; it would merely make it heat more on discharge.

So far the cells considered have been reversible. It does not follow that a cell is always reversible, but, if not, there is at least one non-adjuvant action. As a good example of non-reversibility, aluminium and its solutions may be taken. Aluminium does not dissolve in dilute nitric or sulphuric acid, yet it cannot be deposited electrically from any known solution. Aluminium and carbon in nitric acid give only a small fraction of a volt.

From the definition of electromotive force adopted in this paper, the "seat of the electromotive force" is in the cell and not between the positive and negative metals outside. Yet the electrometer behaves as if there were a contact electromotive force. Dr. Lodge has attempted to explain this in accordance with the "chemical theory." He argues that when, for instance, zinc is exposed to the air, the oxygen either begins to combine with it, or actually combines with it, the particles of oxygen giving up their charge to the zinc on combining. This produces a difference of potential which increases till the oxygen's attraction for the zinc is counterbalanced by it. There is thus an electromotive force set up. Air being an insulator, the circuit is not completed. If the

tendency to combine with oxygen can produce an electric stress which prevents combination, actual combination must either charge the metal, if insulated, or produce a current, if a path is allowed. If a clean piece of sodium is put on an insulating stand it goes on oxidizing, the amount of oxide formed corresponding to an enormous number of coulombs. Where do they go to? If the metal charges electrostatically, it must soon be millions and millions of volts below the potential of the air, and must discharge disruptively. Moreover, the millions of volts are far more than equivalent to Na_2,O. Dr. Lodge assumes that a single element is an electrolyte, whereas a cell can only discharge by double decomposition. That is to say, to produce such an effect the metal must tear the oxygen from a combination, the other radical combining with another less electropositive metal, or removing an electronegative radical from it. It might be argued that zinc and copper plates in chlorine water will give a current, and chlorine is a single radical like oxygen. But immediately the plates are inserted the chlorine combines directly with both metals, without giving any current. A three-fluid battery is thus produced. The zinc is in a solution of zinc chloride, and the copper in copper chloride, and the intermediate liquid is chlorine water. The cell then discharges like a Daniell.

Though the oxygen form of the corrosion theory of contact electromotive force may not hold, it is quite possible that the Volta effect may be produced by thin films of water. Water is even more difficult to get rid of than oxygen, and might easily cause the electrometer readings. It must be remembered in connexion with this, that water must be present to enable even a combination of a metal with chlorine to take place. It is well known that dry chlorine will not attack a dry metal. Even sodium may be left in contact with chlorine.

Even if the Volta effect is due to the presence of traces of an electrolyte, such as water, and not to a non-electrolytic combination with free oxygen, the "seat of the electromotive force" still depends on a mere definition. The term "electromotive force" is continually used in two senses. It is sometimes used to denote the difference between the potentials of two points; and sometimes to denote the rate of fall of potential.

Maxwell gives it the latter definition formally, but frequently writes of the electromotive force when he means the line-integral of the electromotive force, that is to say the difference of potential. In connexion with cells, "electromotive force" is always used to mean difference of potential, so the stricter meaning, rate of fall of potential, may be disregarded just now. If we define the difference of potential between two points as proportional to the work done on a unit quantity of electricity moving from one point to the other, we have a clear statement of what we mean. But if we attempt to make the measurement, we find the unit quantity of electricity must have some carrier; and when we use an electrometer, we do not measure the work done on a unit quantity of electricity moving from one point to the other; we really measure the work done when a particular conductor is used as carrier. It is not necessary to consider any part of the circuit as the seat of the electromotive force. There is a circuit, and the potential is cyclic. Similar cases occur in other branches of electricity. For instance, a "unipolar" dynamo may be made up of a rotating magnet with a stationary circuit. If the resistance of the internal circuit is negligible in comparison with that of the external, the whole expenditure of power is in the external circuit, and the fall of potential over the external resistance is sensibly the whole potential or electromotive force of the machine. There is, however, no way of finding the seat of this electromotive force. If the rotating magnet is supposed to carry "lines of induction" round with it, they cut the external circuit and produce electromotive force there, so that becomes the seat of the electromotive force. If the lines of induction are taken as stationary, the rotating part of the circuit cuts them and becomes the seat of the electromotive force. The effects on the external resistance are the same in both cases. If it is a voltmeter the readings are the same, and they remain so if the lines of induction rotate faster or slower than the magnet, or in the opposite direction. If the voltmeter is replaced by an electrometer, it is still impossible to say where the seat of the electromotive force is. The readings would be the same whether the lines of induction were stationary, or revolved and cut the leads to the electrometer, or revolved and passed through the electro-

meter as if it were a zigzag gateway, without cutting the metallic part of the circuit anywhere. If the electrometer is removed and a proof-plate is used, by touching one terminal with it and measuring the work done when it moves to the other, the result is the same. If the lines are stationary, the proof-plate is charged by one terminal and repelled by it and attracted by the other. If the lines rotate, the proof-plate is urged forward by the lines of induction cutting it at right angles to its path. The reading is the same in both cases. The lines of induction are a mere convention, and there is no way of finding the seat of the electromotive force. All that can be said is that it is cyclic, and that the difference of potential of any two parts of the circuit of the same metal can be measured. In some cases the seat where power is spent can be determined, and in others it cannot.

Similarly in the cell, the seat of expenditure of power can sometimes be told, as when there is a resistance in circuit; sometimes it cannot, as when the cell works a unipolar, or, by extension, any other motor. The difference of potential between any two points of the same material can be measured, but all that can be said is that the electromotive force of the whole circuit is cyclic.

XVIII. *The Theory of Dissociation into Ions, and its Consequences.* By Spencer Umfreville Pickering, *M.A., F.R.S.**

The supporters of the present physical theory of solution hold that the majority of salts, acids, and bases, when dissolved in a large excess of water, are entirely resolved into their component ions. That the facts of the case warrant such a conclusion I have already disputed (Phil. Mag. vol. xxix. p. 490, and 'Nature,' xlii. p. 626); but theoretical objections of a fundamental character can, I believe, be raised against this dissociation theory, and, though I have already alluded to these, they have not yet been fully discussed. Many of the questions which are asked in the following pages can no doubt be answered satisfactorily, but as far as I am aware this has not

* Read March 20, 1891.

yet been done, and it is certainly incumbent on the supporters of the theory to give physicists and chemists a clear conception of what their theory involves.

When hydrochloric-acid gas, for instance, is dissolved in water, the molecules, which were intact to start with, become resolved into their ions, so that each of these acts as if it were a separate unit. This I believe is, according to the theory, the total and only change which occurs: the water remains in the same state in which it was to start with. The resolution of HCl into H and Cl atoms has been held of necessity to involve an *absorption* of heat, an absorption considerably in excess of that which we know occurs when it is resolved into hydrogen and chlorine molecules; and, whether the ions are identical with free atoms or not, we have the positive statement of Arrhenius, the originator of the present dissociation theory, that the resolution of a body such as hydrochloric acid into its ions absorbs heat*. If then this, which is the only change, *absorbs* heat, whence comes the 17,300 cal. which are, as a matter of fact, *evolved* during dissolution?

In the communication to which reference has been made, Arrhenius does not consider the thermal results of dissolution, and a subsequent consideration of these seems to have led some of the supporters of the theory to hold a view diametrically opposed to that just quoted. They now hold, I believe, that the decomposition of molecules into their ions *evolves* heat; that that heat, which they still admit must be absorbed by the decomposition of the molecules into ordinary atoms, is more than counterbalanced by the combination of the atoms with electric charges. This change of front must rather be inferred indirectly from the writings of dissociationists than

* Arrhenius, *Bitrag till Kongl. Svenska Vetenskaps-Akademiens Handlingar*, Stockholm, 1883–4. Out of several sentences I may quote the following italicised passage:—"La transformation de l'état inactif en l'état actif d'un hydrate (faible) est accompagnée par une absorption de chaleur;" and the heat absorbed in the transformation is named "chaleur d'activité." The terms used in the above must be translated into the more modern language of the dissociationists, thus: inactive = undissociated; active = dissociated into ions; hydrate = any hydrogen compound, such as an acid or base; feeble = not much dissociated. See also Lodge's epitome of Arrhenius's paper, B. A. Report, 1886, pp. 361–384.

from any definite retraction which they have published; nor does it appear to have been followed by all the supporters of the theory, for the explanation given by Arrhenius of the constancy of the heat evolved on neutralizing acids with bases is that it is in all cases due to the combination of the ions H and OH to form H_2O, and this explanation was quoted as recently as September last (B. A. meeting) by Shaw as being one of the strongest arguments in favour of the theory. It may also be remarked that up to July 1889 Ostwald seems to have held both views, and to have adopted either just as the exigencies of the case suggested: he explains the normal heat of neutralization as being due to the heat evolved in the formation of a molecule from its ions ('Outlines of General Chemistry,' 1890, p. 368), and the abnormal heat of neutralization as being due to heat evolved in the formation of ions from a molecule, though not, of course, the same molecule as in the previous case (p. 369).

The first point, therefore, on which the dissociationists should give us definite information is, whether the dissociation of a molecule into ions is supposed to evolve or absorb heat.

Presupposing that the answer will be that heat is evolved (at any rate in cases similar to that of hydrochloric acid), their theory cannot be said to be *primâ facie* inconsistent with the conservation of energy; but other very serious difficulties arise which call for explanation.

The idea of heat being evolved by the combination of a charge with an atom involves the conception that the charge is originally independent of the atom: indeed the main idea of the theory seems to lie in the distinction between an ion, or charged atom, and an ordinary or uncharged atom*. We may ask, therefore, whence come these charges? All the ordinary means by which bodies become charged seem to

* *Cf.* Ostwald, *loc. cit.*, p. 275:—" What actually exists in the solution is single potassium atoms with enormous electrical charges. We do not know what those charges are in reality, but this we do know, that the chemical properties of substances are greatly altered by electrical charges. . . . As soon as the potassium atoms in solution lose their charge, as they do, for example, when led by an electric current to a platinum wire, where they can give up their electricity, potassium with its ordinary properties is at once produced, as is seen in its ability to decompose water with evolution of hydrogen."

be absent in the present case. No external energy has been expended, no friction can be supposed to exist except such as might result indirectly from an attraction between the water and the acid ; but even if the existence of such an attraction were admitted, it could never cause sufficient friction to overcome the very attraction which is the original cause of it, to say nothing of the still stronger attraction which holds the atoms together : induction cannot apply, as both water and acid are supposed to be uncharged to start with, and even if there were a contact difference of potential between these two substances, it would not result in communicating both the + and — charges to one only of the bodies brought into contact,—the acid.

In the second place, how can we imagine that an electric charge, which we must at present regard as an affection of matter, can combine with matter to produce heat and itself remain *in statu quo*? Such a view is little less than endowing the charges with some of the exclusive properties of matter, and calling this new matter into existence just when and where may be most convenient to the theory.

In the third place, how can it be maintained that the positive electrification of the hydrogen, and the negative electrification of the chlorine, would dissolve the union between them? According to all our experience of electricity, such electrification would make them cling together all the more firmly. Further, if these so-called + and — charges repel each other, why are they attracted by the — and + charges respectively on electrodes during electrolysis? or why, again, do the similarly charged atoms not attract each other (as dissimilarly charged ones are supposed to repel each other) and form hydrogen and chlorine molecules?

That a molecule, when decomposed by some force superior to the attraction of its constituent atoms, gives rise to free atoms which are possessed of a certain amount of free energy and that this free energy, which we call chemical affinity, may really be of the nature of an electric charge, has received the support of the greatest chemists and physicists whom Science has known ; but the present theory seems to have nothing in common with such a view—indeed, it seems to be directly opposed to it. On the old theory the atoms when separated have *more*

free energy than when combined, on the new theory they have *less*: on the old, the electric charges are the *consequence* of decomposition by some superior force, and form an integral part of the stuff resulting from the decomposition; on the new, they are the *cause* of this decomposition and are something outside and independent of the matter itself. The old theory attributes chemical affinity and combination to the existence of these charges; the new theory considers the charges to be antagonistic to chemical affinity, and to be the cause of chemical decomposition.

The view has been suggested, I believe, that the supposed dissociated atoms, though no longer held together by chemical attraction, may be still held together by the electrical attraction of their charges. This seems to be but an attempt to overcome a difficulty by changing a name, and so far from really diminishing the difficulty, it would appear only to increase it: for heat has been evolved, and, therefore, the state of combination is more intimate than it was before dissolution, so that the matter must be held more firmly together by these electrical charges than it was by its chemical affinity: how does this help the statement that they are *less* firmly united now—so much less firmly according to the theory, that they act as independent units? The difficulties as to the origin of the charges and the antagonism of chemical and electrical attraction are, moreover, not removed by this method of expressing the theory.

Another view, again, was suggested at the recent meeting of the British Association: that, instead of regarding the ions as atoms with electric charges, they might be regarded as allotropic modifications of the atoms themselves. This appears to me to be but hypothecating a new form of matter to satisfy a theory which is inconsistent with known matter, and, inasmuch as atoms of the same substance cannot differ from each other except by possessing different quantities of energy, it practically amounts to the conjuring away a stock of energy that the theory may not be said to be contradicted by the principle of the conservation of energy. But surely such a process is in reality as much a violation of this principle as writing $2=4$ would be. The energy equation will not equate, so the excess of energy on the one side is boldly struck

off by imagining a new form of energyless atom, just as on the electric-charge theory the same is done by saying that the superabundant energy has been expended in combining with charges which have come from nowhere.

Whereas the potential energy of the ions of a substance such as hydrochloric acid must be regarded as less than that of the molecules when gaseous, it would appear that it must be greater than that of the molecules when solid—at any rate in such cases where the solid dissolves with an absorption of heat. Thus, in the case of potassium chloride, 74·5 grams of the salt dissolve in water at 0° with an absorption of 5184 cal. Let P and K represent the potential and kinetic energy of the solid (KCl) and of the ions (K + Cl) respectively, then

$$P(KCl) + K(KCl) = P(K + Cl) + K(K + Cl) - 5284 \text{ cal.}$$

The kinetic energy of the solid, K(KCl), is possibly an unknown quantity, but it is certainly a positive quantity; the kinetic energy of the dissolved substance, K(K + Cl), can, according to the osmotic-pressure theory, be calculated. With a monatomic gas no intramolecular work is possible, and therefore the pressure caused by that gas is a measure of its total kinetic energy (*cf.* Ostwald, *loc. cit.* p. 76); the pressure of each ion is in this case, *ex. hyp.*, the same as that which an ordinary molecule produces, and the pressure-producing energy of a gram-molecular proportion of such a gas is 34,008,000,000 ergs, or 819 cal., so that

$$K(K + Cl) = 2 \times 819 \text{ cal.},$$

$$\text{and } P(KCl) = P(K + Cl) - 3646 \text{ cal.} - K(KCl).$$

Thus the potential energy of the ions must be considerably greater than that of the solid molecule.

The same substance might have been taken as an instance of a gas dissolving with evolution of heat and a solid with absorption of heat; and in such a case the potential energy of the substance in solution must be intermediate between that of the gaseous and solid molecules. It is certainly difficult to imagine that this substance can consist of uncombined atoms.

But a still greater difficulty arises in some cases: a solution of calcium nitrate, for instance, absorbs heat on dilution, and,

as the only change produced by diluting an already dilute solution is, according to the theory, to dissociate into ions some of the molecules still remaining intact, and as these molecules are present in the uncombined and gaseous condition, it follows that the dissociation of gaseous calcium nitrate molecules must absorb heat; therefore the gaseous salt on being dissolved in excess of water must absorb heat, the liquid salt would absorb more than the gas (by an amount equivalent to the heat of vaporization of this salt), and still more would be absorbed by the solid; yet direct experiment shows that this last, instead of giving a large absorption, actually *evolves* 4000 cal. when it is dissolved.

The conception that salts &c. in solution are entirely decomposed into ions has been regarded as a development of the theory of Clausius that a few free ions exist at any given moment in a mass of solution. But these two views appear to me to be radically different. Clausius's conception (Phil. Mag. 1858, vol. xv. p. 100) was of a two-fold nature. (1) That the molecules in any fluid, being at different temperatures at different times, owing to the impacts to which they have been subjected, some of them may occasionally be so hot as to be above their dissociation temperature, and that some temporarily free atoms would therefore be present. (2) That two similar molecules MR and M′R′ might collide under such circumstances that M′ might come nearer to R than M was, and, consequently, M′R would be the result; whether, however, M and R′ would find themselves in such close proximity that they would instantly combine together, or whether they might remain for a time actually free, is a question which our ignorance of the distances and forces concerned do not allow us to settle. Williamson's theory (Phil. Mag.) was practically identical with Clausius's second proposition—continual interchange of radicals, but not necessarily the presence of free atoms. Clausius's theory was proposed to account for the facts of Electrolysis, Williamson's to account for Chemical facts. Nothing which has come to light since that time seems to have shaken the idea of a continual interchange of atoms in the molecules of a liquid; there seem, however, to be decided objections to the conception of free atoms being present. Whether it is probable that in a liquid at ordinary temperatures there can be any mole-

cules as hot as the dissociation temperature (which is probably 1000° to 2000°) or not, is a matter of opinion; but if any free atoms are present the chances of their meeting atoms of the same nature would be equal to those of their meeting atoms of the opposite nature: with the former they would combine just as they do with the latter, and the result would be that hydrogen and chlorine gas would be formed, and the acid would gradually become entirely decomposed: nor can it be argued that the similar charges on the free atoms of the same substance would prevent these combining, for in the analogous case of hydriodic acid we know as a fact that free iodine is produced when the acid is heated to its dissociation temperature.

It does not seem necessary, however, to imagine the presence of free atoms to explain the phenomena of electrolysis (*cf.* Lodge, B. A. Report, 1887, p. 338). The facts of the case, I believe, are that although an E.M.F. of finite magnitude is required to produce sensible electrolysis, *i. e.* the liberation of gas, &c. in recognizable quantities, any electromotive force will produce results indicative of electrolysis, these results being a gradual leakage of electricity, and a reverse or polarization current on removing the battery. The leakage may be explained by the electrolysis of those molecules which happen to be at a temperature above that of the average molecule, and decomposable by a lower E.M.F. than is the average molecule, or else by the action of the E.M.F. on those dissimilar atoms which at the moment when they lose their original partners find themselves comparatively very far apart from each other (M and R′ in the case above cited). As to the polarization current I venture, though with considerable diffidence, to ask whether it really is evidence of actual electrolysis. It is explained by the statement that a coating of liberated ions is formed on the electrode, and that these cannot be discharged so as to become ordinary atoms till the potential of the electrode attains a certain value, they consequently remain there and cause a reverse current. But might not such a current be caused by a similar coating of simple but undecomposed molecules; such molecules are, I believe, not fully saturated compounds, but still possess a certain amount of residual affinity, or, on the older electro-

chemical theory, a certain amount of unneutralized charge; these would be attracted to the electrodes and would present their + and − ends to the − and + electrodes respectively: a charge would thus be retained on the electrodes after the battery was disconnected, and this charge would cause a reverse current when the two electrodes were connected together. The action in fact would be similar to that in a condenser. Such an explanation may obviate objections which can, perhaps, be urged against the idea of a coating of ions, for it seems difficult to see why a certain E.M.F. should be reached before the atoms can discharge themselves, unless we imagine a definite attraction between an atom and its charge, or why the E.M.F. required to effect this discharge should not always be the same whenever the same element is liberated; it also obviates the necessity of regarding an ion as possessing any form of charge which a free atom does not.

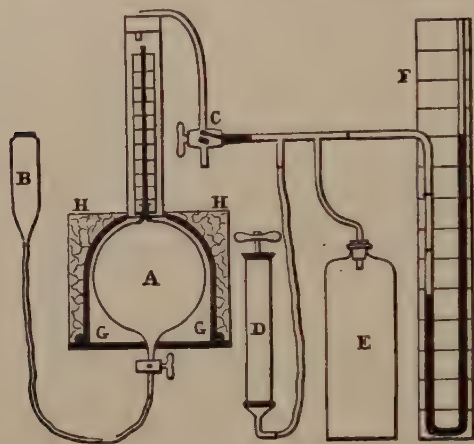
Whatever be the value of these suggestions, and of the objections raised against that part of Clausius's conception which supposes the presence of a *few* free atoms in a liquid owing to the accidental superheating of some of the molecules, it must be borne in mind that this view is totally distinct from the modern dissociation theory, that *all* the molecules are dissociated, and that too not by heat but by their affinity for electrical charges of inscrutable origin, and possessing hitherto unknown characteristics.

XIX. *An Apparatus for Measuring the Compressibility of Liquids.* *By* S. Skinner, *M.A., Demonstrator at the Cavendish Laboratory, Cambridge**.

The special features of the apparatus are (*a*) its very large bulb, (*b*) an arrangement to facilitate the filling and emptying processes. The volume of the globe is 1300 cubic centimetres, so that a small alteration of pressure, such as half an atmosphere, produces a considerable movement of the index in the capillary tube of which the capacity per cm. is ·003 cubic centim. To facilitate the introduction of liquids

* Read May 9, 1891.

a second tube, closed by a stop-cock, is sealed in at the lower portion of the globe, and this is connected by a rubber-tube with a side reservoir. If a liquid be poured in the reservoir



and the stop-cock be opened, the liquid will flow into the globe. The apparatus is supported on a base-board through which the tube carrying the stop-cock passes. A bell-jar stands on the base-board and is in connexion with a condensing air-pump and a mercurial pressure-gauge.

The behaviour of the apparatus has been tested by using it for a determination of the compressibility of water, with the following results :—

	Found.	Calculated from Tait's Extrapolation Formula.
At 5°·45 C.	·0000510	·0000502
9°·4	·0000501	·0000488
12°·4	·0000485	·0000479
16°·2	·0000480	·0000468

The apparatus has been applied to the comparison of the compressibility of solutions with that of the solvents, and the results exhibit the same general relations as with other properties of solutions. The solutions divide themselves into two classes, which may be broadly termed electrolytic and non-electrolytic. In the first there is a very considerable decrease in compressibility, as much as 8 per cent. with a 3-per-cent. solution of NaCl; whilst in the second there is

but a slight diminution, quite beyond the limits of accuracy of the apparatus. For instance, a 5-per-cent. solution of naphthalene has a diminution of under 1 per cent.

A strict interpretation of the theory of osmotic pressure in solutions has been made by Prof. J. J. Thomson, in his "Applications of Dynamics to Physics and Chemistry," § 97, where he finds that 1 gram-equivalent per litre would decrease the compressibility by 1 part in a thousand. Non-electrolytes in dilute solution appear to follow this law; this is in agreement with their other properties, such as alteration in boiling- or freezing-points.

The diagram will serve to explain the general arrangement of the apparatus.

XX. *Note on* Kohlrausch's *Theory of Ionic Velocity. By* W. C. Dampier Whetham, *B.A., Coutts Trotter Student of Trinity College, Cambridge* *.

Kohlrausch calculates his numbers for the specific velocities of different ions from measurements of the conductivities of salt-solutions, and of their migration constants, on the supposition that all the molecules of the salt present in solution are actively concerned in conveying the current. The values thus obtained were found to agree with experiment in certain cases by Prof. Oliver Lodge, and an investigation I am now engaged in carrying out seems also to confirm them. It seems to be generally supposed that this is inconsistent with any theory (such as that of dissociation) which supposes only a certain part of the salt to be active (see Lodge, B. A. Report, 1886, p. 391), though some such form of theory seems to be required by the relations shown to exist by Arrhenius, Van't Hoff, Ostwald, and others. If we examine the matter a little more closely, however, I think the two suppositions can be reconciled. Suppose that the ratio of the numbers of the active and the inactive molecules (which is generally supposed to measure the "dissociation") represents in reality the average ratio of the time during which each molecule is active to the time during which it is inactive. Every mole-

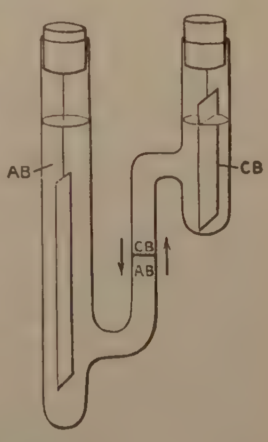
* Read May 9, 1891.

cule is in turn active, but *at any instant* only a certain fraction of the molecules are active. [In terms of the dissociation hypothesis, the dissociation ratio measures the ratio of the mean free time to the mean paired time of the ions.] This is, of course, equivalent to supposing a certain fixed fraction of the whole number of molecules to be active, as far as statical effects, such as osmotic pressure, are concerned, but when we consider the velocities of the ions the case is different.

Kohlrausch calculates the relative velocity of the two ions $U=u+v$ from the molecular conductivity k/m, where k = specific conductivity of the solution, and m its contents in gramme equivalents of salt, $U_1=u+v=k/m$. If now we suppose that at any instant only $1/n$th of the number of molecules are active, we should apparently have to put $U_2=\frac{kn}{m}$ *in order that the same current may be carried*, which would give $U_2=nU_1$.

But this U_2 represents the actual velocity of the ions while they are "free," and if we take a "dynamical" view of the dissociation equilibrium, they are only free for $1/n$th of their time; while combined they have no relative velocity, and so their *average* velocity for any *long* time is $\frac{1}{n}U_2=U_1$, the same as on Kohlrausch's hypothesis.

The investigation alluded to above, at which I am now working, seems to yield excellent results for certain cases, though it is of somewhat limited application. It consists in observing the phenomena at the junction of two salt-solutions, one of which is differently coloured to the other, when a current of electricity is passed across it. Salts are chosen which have one ion in common and the other different. Let us represent them by AB and CB, and consider the junction phenomena. The effect of the molecular interchanges will be a motion of B ions in one direction, and a motion of C ions and of A ions in the other.



When a C ion crosses

the boundary, it again forms CB, but the colour of CB is different to that of AB, hence the boundary between the colours will move.

The method will be discussed when more experimental results are obtained; it appears that by measuring the rate of this motion the velocity of the ions can be arrived at. The present is merely a preliminary communication in explanation of the experiments shown to the Physical Society of London on the occasion of their recent visit to Cambridge.

XXI. *A Steam-Engine Indicator for High Speeds.* *By* Prof. John Perry, *F.R.S.**

Members who are not practically acquainted with the errors of the ordinary steam- or gas-engine indicator are referred to a paper in the 'Proceedings of the Institution of Civil Engineers' (vol. lxxxiii. 1885) by Professor Osborne Reynolds, and an exhaustive discussion of the subject.

Whether we do or do not share Prof. Reynolds's view that even at low speeds of engines the errors in calculating horse-power are very considerable, because of the friction of the paper barrel and the stretching of the cord or wire which gives to the paper barrel a miniature motion of the engine piston, it is obvious that there will be more confidence in an indicator in which the motion-copying mechanism gives a very short stroke, is opposed by very small forces, and requires no string or wire. I do not pretend to get rid of the error in a spring which is due to change of temperature, but where great accuracy is required it is always worth while, when using this or any other indicator, to have a "boiler-pressure" line drawn upon the diagram, as well as an "atmospheric line;" and these two lines, with a pressure-gauge on the boiler, will give the scale to which the indicator at the time represents pressure and prevent error due to change of temperature of the spring.

In these days of high-speed machinery the most important defect of an indicator is its slow natural period of vibration.

* Read May 22, 1891.

If the natural period of vibration is from $\frac{1}{20}$ to $\frac{1}{30}$ of the time of revolution of an engine, it is found that the diagram is not deformed by waviness, ordinary fluid friction destroying the natural vibration; if it is as much as $\frac{1}{15}$, a considerable amount of pencil pressure must be employed to obtain a frictional stilling of vibration; but if it is as low as $\frac{1}{10}$ it is almost impossible, even with great pencil friction, to obtain a decent diagram. To make an indicator have a quicker natural vibration it is necessary to use a stiffer spring, and this means that pressures are indicated to a very small scale. Introducing seven per cent. of inaccuracy in area of diagram, either by friction or smallness of scale, may possibly allow the very best existing indicator to be used on engines of as high a speed as 400 revolutions per minute; but this is a matter on which assertions are made of higher and very much lower limits of speed than what I have stated. I think that users of indicators will generally agree that I have not exaggerated this defect; but unfortunately there is scarcely ever any kind of check which can be applied to a measurement of indicated horse-power, so that mere assertions are of very little value.

Indicated horse-power is now the sole standard of the values of engines, so that its accurate measurement is very important; but for the inventor and improver of engines, and for the physicist, that there shall be no local deformations in the shape of the indicator-diagram is of greater importance than that its area should be correct. Now the natural period of vibration of the indicator before you is about $\frac{1}{500}$ of a second; allowing twenty periods to one revolution of the engine, I find that this indicator will give diagrams with no wave-deformations until the speed of the engine exceeds 1500 revolutions per minute. I can, however, by changing the disk make the natural period of vibration $\frac{1}{1000}$ or $\frac{1}{250}$ of a second, or, indeed, what I please, so that there is practically no speed at which this indicator will not give a true diagram.

Specimens of the indicator are before you, but I will throw upon the screen drawings of it. Fig. 1 is a section, and fig. 2 an elevation. It consists of a very shallow circular box, E, of metal (in some of my specimens it is of cast iron, in others of gun-metal) closed by a thin disk of steel, D.

Here, for example, is one an inch and a quarter in diameter, and about $\frac{1}{60}$ of an inch thick, which I use for maximum steam-pressures of about 30 lb. to the square inch above the atmosphere. It was my intention to use corrugated disks, but as these could not be obtained cheaply, except in quantity, I have hitherto used plane disks, and found them quite satisfactory for such sizes of diagram as I have hitherto dealt with. I am, however, making arrangements for obtaining corrugated disks for use in the indicator.

Fig. 1.

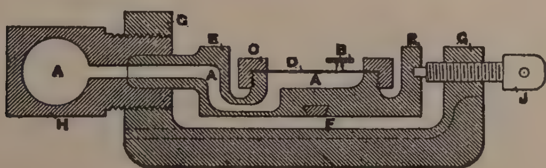
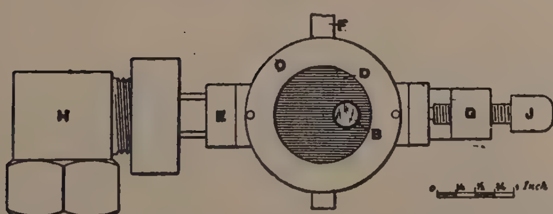


Fig. 2.



Now when this box E is put in communication with the cylinder of a steam-engine in the usual way, that is through the pipe A and the indicator-cock, the disk yields more or less as the pressure is greater or less. To magnify this yielding I fix upon the disk, about halfway between its centre and circumference, a small mirror B such as is used in electrical laboratories. This mirror has a light frame of metal, and can be screwed to or unscrewed from any of these disks quite readily. I let a beam of light from an ordinary oil-lamp fall upon the mirror, and it is reflected and falls upon a sheet of white paper which it illuminates at a small spot. Now the yielding of the disk under fluid pressure is evidenced by the movement of the spot of light on the paper. For example, if steam of 10, 20, or 30 lb. pressure per square inch (above the atmosphere) is admitted to this box with its

present disk, and the spot of light is on a sheet of paper about 4 feet away from the mirror, the spot will be seen to travel one, two, or three inches from its original position. If there is a partial vacuum inside the box, the spot travels in the opposite direction. It will be seen, therefore, that I have used a reflected beam of light as if it were a rigid pointer four feet long. By using a lens, by using magnesium light instead of a common oil-lamp, and by taking certain precautions which are quite obvious to the members of this Society, I could throw a well-defined spot upon a screen forty feet away from a mirror upon a small corrugated disk, whose motion would be with great exactness proportional to the pressure, for motions of as much as five or six feet.

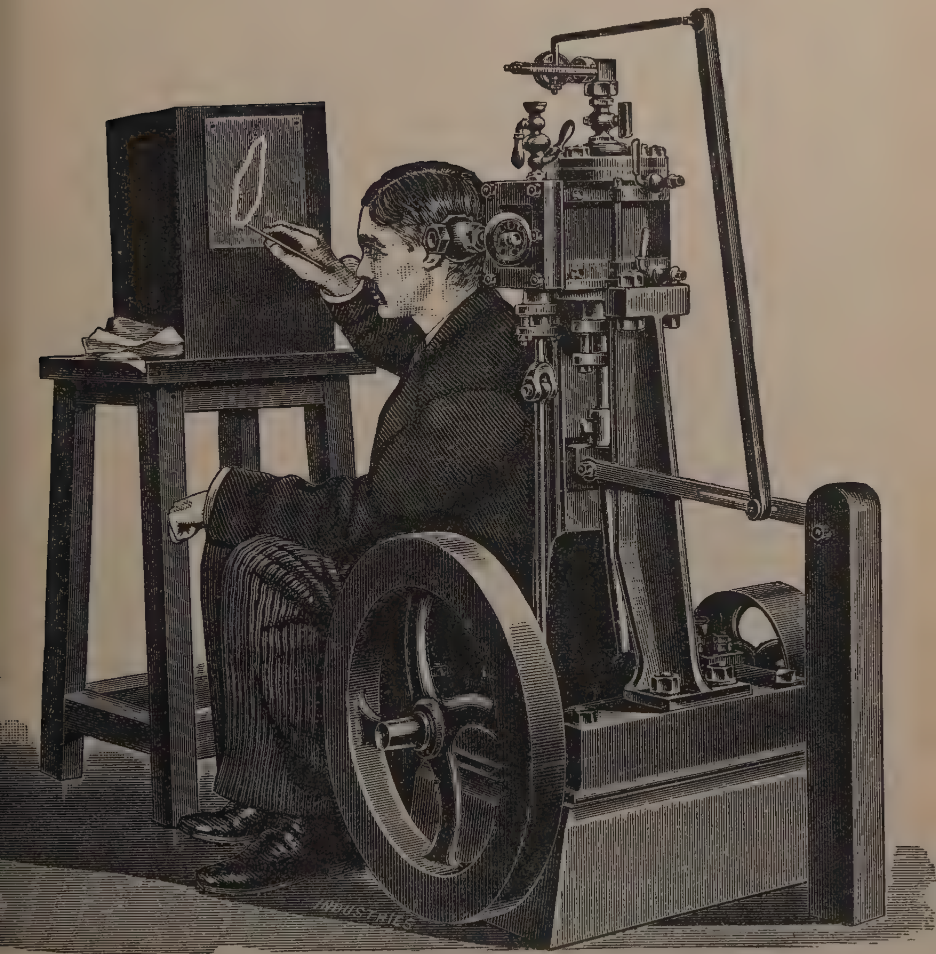
The end of the arm F receives a miniature motion of the piston of the engine by stiff rods, and this causes the spot to move at right angles to its former motion; and when both motions are being given the spot travels round on the screen, its position at any instant indicating the pressure of the steam and the position of the piston in its stroke.

Now, although I have never heard of such a method being used, I feel that many people must have thought of using it. I myself thought of it many years ago; but the making of a photographic record seemed to me to introduce great difficulty, so I never tried it*. What I have now discovered is this, that a photographic method of recording is quite unnecessary. In fact, even at speeds of 60 revolutions per minute the image of the spot remains on the retina sufficiently long to enable a man to draw upon the screen the path of the spot. He first turns the indicator-cock, so that there is atmospheric pressure under the disk. The spot now travels in a straight line, and this is the atmospheric line. When I wish to check the scale I do what I should like to see done with all indicators, I let steam at boiler-pressure underneath the disk and mark out another straight line parallel to the atmospheric line. I now let the box communicate with the cylinder, and I draw the actual diagram. A very little practice will enable anyone to

* Prof. R. H. Smith has, since the reading of the paper, called my attention to an indicator described in 'Engineering' of July 10th, 1885, by Messrs. Clarke and Low, in which a reflected beam of light is used for magnification.

draw the diagram quite accurately, even when the engine makes only 60 revolutions per minute. But at such speeds as 150 revolutions per minute, the diagram is quite continuous

Fig. 3.



as a thin line of light on the paper, and the most unskilled person need not make errors of as much as one per cent. in drawing a pencil line, which remains quite visible in the middle of the thin line of illumination.

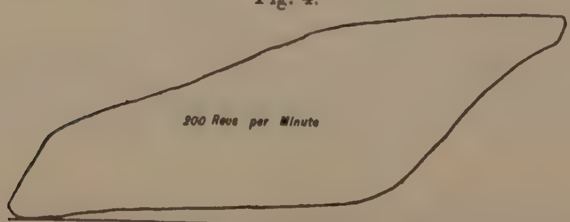
Using a common oil-lamp, a diagram about six inches long and four inches broad, formed of a band of light one tenth of an inch broad, is quite visible even in a well illuminated room. If the room is darkened the diagram becomes quite vivid, and to anyone accustomed to indicator-diagrams it is an object of interest from quite a number of considerations.

As I am unable to show to the Society this instrument in action, I will throw upon the screen a photograph, fig. 3, taken at Finsbury of the indicator placed upon a toy steam-engine, which some of my students made to drive testing machines. My partner, Mr. Holland, is following the spot of light with a pencil. As a matter of fact, the engine was really at rest, and the diagram was drawn as a thick chalk line upon dark paper, when this photograph was taken; but if an instantaneous photograph had been taken of the arrangement actually working, it would not have been very different from what I here show, except that the line of light forming the diagram would have been finer.

We have only been able to run this toy engine at a maximum speed of about 900 revolutions per minute, and indeed at this speed it was rather dangerous to run it, as, through want of balance, it set the floor and everything in the room a-shaking; but even at this high speed there was no evidence on the diagram of any waviness due to the natural vibrations of the disk of the indicator. There was, however, a musical note in evidence of the existence of such vibrations.

Mr. Holland has used a tapering wooden box as if it were a camera for photographing the diagram and its atmospheric line, and he tells me that it is quite easy to take

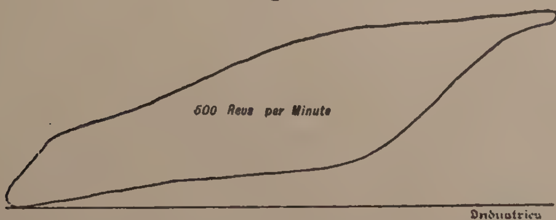
Fig. 4.



such photographs. He is, however, accustomed to such work, and I do not think that an ordinary engine-driver would care

to try the photographic method. I here exhibit some of Mr. Holland's photographs. This one, fig. 4, required an exposure of one minute, the light being that of an ordinary

Fig. 5.



oil-lamp. This one, fig. 5, required an exposure of ten seconds, the light being obtained by the burning of a piece of magnesium strip behind the hole.

I have, however, used Mr. Holland's box with sheets of tracing-paper instead of photographic plates, and the indicator-diagram is now an object which is very easily observed and traced.

I exhibit a few drawings made on tracing-paper in this way by Mr. Holland. First, we have a few taken at about the same time; these taught us that we had better make considerable changes in the valve of our engine. We made these changes, and here are some others taken subsequently. The speed and other necessary information are given on each sheet of paper, so that the effect of change of speed on our toy engine may be noted.

With an ordinary indicator the instrument must be stopped in its action, and a sheet of paper taken from the drum, before the diagram can be looked at. But here the diagram is visible all the time. To the student or improver of steam-engines it is very instructive to keep looking at the diagram whilst altering steam-pressure, or speed, or load of the engine, and it is an amusement of which one does not very soon become tired. Even with this toy engine of mine I have already observed such changes in the shape of the diagram as have thrown a perfectly new light upon the condensation phenomena occurring inside a steam-engine cylinder.

The first form of this new indicator is lying on the table. You will see that to give it the piston motion in miniature

required considerable force to be exerted, a fault which I have corrected since. Now the very first time the instrument was tried a very curious phenomenon was observed, namely, that the indicator-diagram was not one continuous line of light, but a series of "blobs" or spots, connected by a much fainter line. I easily saw that these indicated a vibration going on in the rods which gave to the box the motion of the piston in miniature. I found that the cause of this was due to the bad fitting or "backlash" in a lever which my students had been in the habit of using with ordinary indicators on the same engine. With an ordinary indicator there was no possibility of observing from the diagram that such a fault existed, and was probably greatly accentuated by the use of a cord instead of our rod; here it was very evident. Indeed, although only to a small extent, it will be observed in the photographs that there are regular changes of intensity of light in every diagram, and that this vibration of the piston-motion mechanism has not been altogether done away with even in our more carefully fitted stiff rods.

Added, June 5th, 1891.

In the discussion which followed the reading of the paper it was suggested by Mr. Swinburne that the most useful function of such an indicator would be to continually show the diagram on a screen in an engine-room, so that at every instant the engineer would not only know the pressure of steam in his boiler, but also the pressure at every instant in the cylinder.

I quite forgot to put this in my paper, although its importance was not unknown to me. I am now attaching an indicator to each end of the cylinder of my driving-engine at Finsbury. These will receive their miniature piston-motion from one rod. One gas-jet will throw from the two mirrors two diagrams upon a screen some seven feet away from the mirrors, and these diagrams will be visible from nearly every part of the engine-room at all times. In the room at present there are not only very visible pressure- and vacuum-gauges and a speed-indicator (not a counter), but also a dynamometer coupling, which shows at a glance the actual horse-

power which is being transmitted along a shaft. If, instead of tracing the diagram with a pencil, I were to trace it with the point of a planimeter, the reading might represent the horse-power.

It is unnecessary to describe here how the indicator might be fixed to the valve-chest or receivers, or to other parts of an engine, such as the air- and force-pumps; or to pumping-machinery generally; or to engines driven by fluids. In fact it may be said that I have made the discovery that the beautiful method of observation of vibration of M. Lissajous has hitherto been neglected in its applicability to all sorts of practical purposes. Quite a number of other applications of the reflected beam of light principle suggest themselves.

XXII. *On the Value of some Mercury Resistance Standards. By* R. T. Glazebrook, *M.A., F.R.S., Fellow of Trinity College, Cambridge* *.

In a paper read before the Physical Society † on May 23, 1885, I described the results of a comparison between the original standards of the British Association and some copies of the mercury unit representing Legal Ohms made by M. Benoit in Paris and sent to me by him. Two of these copies have remained in my possession since that time; the third, at M. Benoit's request, was handed to Mr. Preece. The mercury with which the tubes were filled in 1885 has remained in them since that date. Now recent observations had seemed to indicate that a small change had taken place in some of the platinum-silver standards, and it became desirable to compare them again with M. Benoit's tubes. The method was the same as that of my former paper. The tubes immersed in melting ice were compared with the B.A. standards. The temperature of the room was kept very low, from $0^{\circ}{\cdot}5$ to 3° C., and thus the errors caused by the conduction of heat into the tubes through the copper connecting rods were avoided. The connecting pieces used to connect the tubes to the bridge

* Read May 9, 1891.

† Glazebrook, Phil. Mag. October 1885; Proc. Physical Society, vol. vii.

differed from those employed in 1885, being a modification of the platinum cups described in my paper* on the specific resistance of mercury. A hollow platinum cup, about 3·5 cm. long and rather more than 1 cm. in diameter, is secured firmly into an ebonite tube; the outside of the tubes are turned to fit the ground portion of the glass vessels which form the ends of the mercury tubes, thus taking the place of the stoppers which usually close the tube; the platinum cups dip into the mercury, the surface of contact being about 12 sq. cm. The cups had previously been platinized; on the present occasion they were merely cleaned with nitric acid and distilled water. Stout copper rods, well amalgamated, fit tightly into the inside of the cups; the other ends of these rods are in contact with the bridge. Thus the connexion with the bridge is made through the rods and across the platinum of the cups, avoiding contact between the mercury and copper.

The resistance of the connexions was determined, and it was shown that the resistance of the platinum was negligible. This was done by making the rod and cup part of a circuit, the resistance of which was measured. The rod was then removed from the cup, and the latter was removed, so that the part of the copper which had been in the cup was now in direct contact with the mercury into which previously the cup had dipped. No change in the resistance of the circuit was produced by this. To secure this result it was necessary that a considerable area of the platinum should dip into the mercury; and this condition was in the experiments always carefully attended to. The resistance of these connecting-pieces was thus found to be ·00291 B.A. unit.

The tubes were compared both with the B.A. units and also with the Legal-Ohm standards. In the comparison with the B.A. units, a large amount—about 300 divisions, or ·015 B.A. unit—of the bridge-wire was used. Since the resistance of the bridge-wire is known at 15°, this required, when the temperature of the room was as low as 1° or 2°, correcting for temperature; but as the whole correction amounted to less than 2 per cent. of the bridge-wire used, while this again was only 1·5 per cent. of the resistance measured, no very accurate knowledge of the temperature of the wire was needed. In

* Phil. Trans. 1888.

some cases a coil of 100 B.A. units was put in multiple arc with the mercury tube; and the resistance of the combination, which amounted to a little over 1 B.A. unit, was compared with the wire standard.

The results of the comparisons are given in the following Tables.

TABLE I.—Tube, Benoit No. 37.

Date.	Standard.	Notes.	Value in B.A. units.
Jan. 3......	Flat.	....	1·01111
„ 5......	Flat.	....	1·01103
„ 5......	Flat.	100 B.A. units in multiple arc with tube.	1·01108
„ 6......	Flat.	....	1·01099
„ 6......	Flat.	Mercury drawn through.	1·01101
„ 7......	Flat.	....	1·01109
„ 7......	Flat.	Mercury drawn through.	1·01109
„ 7......	Flat.	After interval of 1 hour.	1·01106

Mean for No. 37...... 1·01106 B.A. units.

TABLE II.—Tube, Benoit No. 39.

Date.	Standard.	Notes.	Value in B.A. units.
Jan. 3......	Flat.	....	1·01053
„ 5......	Flat.	....	1·01058
„ 5......	Flat.	100 B.A. units in multiple arc with tube.	1·01060
„ 6......	Flat.	....	1·01049
„ 6......	Flat.	Mercury drawn through.	1·01037
„ 7......	Flat.	....	1·01041
„ 7......	Flat.	Mercury drawn through.	1·01034
„ 7......	Flat.	After interval of 1 hour.	1·01029
„ 8......	Flat.	....	1·01022
„ 8......	Flat.	Mercury drawn through.	1·01027
„ 8......	Flat.	After interval of 1 hour.	1·01033

Mean of last seven for No. 39...... 1·01032.

With regard to the observations on No. 37, it will be seen that they are extremely close, the greatest difference between any two is ·00012 B.A. unit, or rather more than 1 in 10,000; and no appreciable change was produced by passing the mercury through the tubes. The observations on No. 39 are not quite so close. On January 6 a distinct change was caused

by passing the mercury through; after that date the results are fairly consistent, and no further alteration was observed; the mean of the values found after this date may be taken as the resistance of the tube to an accuracy of about 1 in 10,000.

The two results therefore are:—

No. 37............... 1·01106 B.A. units.
No. 39............... 1·01032 B.A. units.

The observations in 1885 were reduced to Legal Ohms, using the value for the resistance of mercury in terms of the B.A. unit adopted by the B.A. Committee in that year, and based on Lord Rayleigh's experiments. According to this 1 Legal Ohm = 1·0112 B.A. units.

If we take these values we have as the resistances in Legal Ohms the following:—

	Benoit.	R. T. G. 1885.	R. T. G. 1891.
37	1·00045	·99990	·99986
39	·99954	·99917	·99913

Comparing the last two columns we see that the tubes have apparently fallen in value relative to the standard Flat in the $5\frac{1}{2}$ years by ·00004, but this quantity is too small to be really certain of. The results of the experiments therefore prove within this limit that the platinum-silver coil Flat has not altered relative to the mercury-tubes in this interval. The difference between the values found by Benoit and myself depends on the value used for the resistance of mercury in B.A. units. According to Lord Rayleigh *, the resistance of a column of mercury 100 cm. long, 1 sq. mm. in section, at 0° C., is ·95412 B.A. unit, and this is the value which has been used in the above. The value found by Mr. Fitzpatrick † and myself for this resistance was ·95352. Practically the same number has been obtained by Wuilleumeier ‡, Hutchinson §, and Salvioni ‖. If this number be adopted, then

$$1 \text{ Legal Ohm} = 106 \times \cdot 95352 \text{ B.A. unit.}$$
$$= 1\cdot 01073 \text{ B.A. units.}$$

* Phil. Trans. 1883.

† Phil. Trans. A. 1888.

‡ Wuilleumeier, *Comptes Rendus*, cvi., 1888.

§ Hutchinson, Johns Hopkins University Circulars, 1890.

‖ Salvioni, *Rend. della R. Acad. del Lincei*, vol. v. fasc. 7.

And then we find as the values of the tubes in Legal Ohms:

	Benoit.	R. T. G. 1891.
37	1·00045	1·00033
39	·99954	·99959

and these numbers show an extremely close agreement, thus proving that the resistance of a column of mercury 100 cm. in length, 1 sq. mm. in section, as given in this indirect way by Benoit's experiments, is very close to the value ·9535 B.A. unit.

In my previous paper * are given some values for the change of resistance of mercury with temperature. On January 3 I observed the resistances of the tubes Nos. 37 and 39 at a temperature of 12°·9 C. The results were:—

	Resistance at 12°·9.
37	1·02254
39	1·02193

From these we get as the values of the coefficients per 1° C., ·000875 and ·000870 respectively. Thus the mean coefficient between 0° and 13° is ·000872. This agrees well with the values found in 1885, the mean value between 0° and 10° being ·000861, and between 0° and 15° ·000879. From these we have as the value between 0° and 13°, ·000872.

Benoit † and Strecker ‡ give higher values than the above, Strecker's value between 0° and 13° being ·000906.

In the paper on the specific resistance of mercury, Phil. Trans. A, 1888, I have called attention to some of the consequences of this difference. The value of the coefficient between 0° and 10° found by Mr. Fitzpatrick and myself in 1888 was ·000876. Kohlrausch § and Strecker || find as the values for the resistance of mercury in B.A. units, ·95338 and ·95334 ; these are less than the value found by Salvioni, Hutchinson, Wuilleumeier, and myself, and the difference is, I believe, mainly due to the uncertainty in the temperature coefficient. They both worked with their tubes in a bath at

* Phil. Mag. October 1885.

† Benoit, *Journal de Physique*, 1884.

‡ Strecker, Wied. *Annalen*, vol. xxv. p. 475.

§ Kohlrausch, *Abhand. der K. bayer. Akad. der Wiss.* ii. Cl. xvi. Bd. iii. Abt.

|| Strecker, Wied. *Annalen*, vol. xxv. p. 475.

about the temperature of the room, and reduced their results to 0° by the use of Strecker's coefficient. The mean temperature adopted by Kohlrausch was 10°, he puts

$$\sigma_0=\frac{\sigma_{10}}{1\cdot 00904},$$

σ_0 and σ_{10} being the resistances at 0° and 10°. According to my own results in 1888 we should have

$$\sigma_0=\frac{\sigma_{10}}{1\cdot 00876};$$

and the value of σ_0 would be increased from ·95334 to about ·95360, which is rather greater than my value. The value ·000876 is given by Benoit for the mean coefficient between 0° and 10°.

XXIII. Mr. Blakesley's *Method of Measuring Power in Transformers.* *By* Prof. J. Perry, *F.R.S.**

Mr. Blakesley's method of measuring the power given to the primary coil of a transformer becomes more important the more it is studied. Mr. Blakesley proved it to be correct if currents followed the simplest periodic law; if there was no magnetic leakage; if magnetic permeability was constant. Any person who has used Fourier's theorem knows that if Mr. Blakesley's rule is right for a sine function, it must be right for any periodic function whatsoever; as any periodic function may be expressed in sine functions, and each of these enters into the equations as if it were alone†.

* Read May 22, 1891.

† This assertion was challenged in the discussion. Perhaps I ought to have explained myself more fully. At the time I happened to be working with Fourier's Series very much, and I lost sight of the fact that what was very evident to me might not be evident to others.

If $$x=\sum_1^\infty (a_i \sin ikt+b_i \cos ikt),$$

and $$y=\sum_1^\infty (\alpha_i \sin ikt+\beta_i \cos ikt),$$

where $k=\frac{2\pi}{\tau}$ and τ is the periodic time, then the average value of xy

Prof. Ayrton and Mr. Taylor have proved the method to be correct for currents of any periodic law, the permeability varying in any way whatever. But they make the assumption that there is no magnetic leakage.

I believe that it was Dr. Hopkinson who, in his paper read before the Royal Society on March 10th, 1887, first departed from the old-fashioned way of treating mathematically the equations concerning currents in neighbouring coils, and he has been followed by everybody else who has written upon that subject since. Some writers of eminence have given, and incompletely, Hopkinson's investigation, evidently not having seen his paper. In my opinion the usual method is somewhat misleading. Assuming no eddy currents in the conducting part of a transformer, the equations written in the old-fashioned, and in what I venture to say is the only correct way, become

$$\left.\begin{aligned} V &= RC + L\dot{C} + M\dot{C}' \\ 0 &= R'C' + M\dot{C} + L'\dot{C}' \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Here V is the voltage at the terminals of the primary circuit, R its resistance, C its current, and L its coefficient of self-induction. R′ is the resistance of the whole secondary circuit, in which we assume no independent electromotive force; C′ is its current, L′ is its coefficient of self-induction, and M is the mutual induction between the two circuits. It may be well to state that, using amperes, volts, and ohms:—If P and S are the numbers of windings of the primary and secondary respectively; if a is the cross section of the iron

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between the limits 0 and τ is

$$\tfrac{1}{2}\Sigma(a_i\,\alpha_i+b_i\,\beta_i),$$

and does not involve any term such as $a\ \alpha_r$ or $b_i\ \beta_r$; that is, into the expression for the average value each term of the Fourier's Series enters just as if there were no other terms. Nearly all practical Electrical Engineers are in the habit of ignoring calculations which assume that a current is a sine function of the time; they say that such calculations are useless because the current never is a true sine function of the time. I have here given one of many examples which might be given in which a proposition concerning any periodic function need only be proved for one of the Fourier terms of that function. And in all cases, the result of the study of a sine function is at once applicable to any periodic function whatsoever.

in square centimetres, λ the average length of the complete iron magnetic circuit, and μ the permeability (being about 1500 in ordinary transformer working), we may take it that

$$L = P^2 \frac{a\mu}{\lambda} \frac{4\pi}{10},$$

$$L' = S^2 \frac{a\mu}{\lambda} \frac{4\pi}{10};$$

and if there were no magnetic leakage—that is, if all the field due to a primary current through every single winding of the primary passed through every single winding of the secondary, then $M = \sqrt{LL'}$, or

$$M = PS \frac{a\mu}{\lambda} \frac{4\pi}{10}.$$

But there is always some magnetic leakage, and it fills me with astonishment that so many investigators should assume that a little leakage makes no difference.

To get an idea of the importance of even a little leakage let us eliminate $\dot{C}_1$ from equations (1) and we have the result

$$V = RC - R' \frac{M}{L'} C' + \frac{LL' - M^2}{L'} \dot{C}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The usual assumption that if LL' only differs by a very little from M^2 the error is unimportant, is seen to be inadmissible when we consider how great a value $\dot{C}$ sometimes may have in comparison with C or C'. Thus, for example, in a transformer with which I have had something to do experimentally, $L = 15$, $L' = 0{\cdot}15$, $R = 10$, and M is very nearly $1{\cdot}5$; so that (2) becomes

$$V = 10C + 10R'C' + \left(15 - \frac{M^2}{L'}\right)\dot{C}.$$

Now, to take the very simplest kind of periodic current, and the one for which the above wrong assumption is least wrong, and a frequency of 106 per second—writing, in fact,

$$C = A \sin 1000\, t,$$

we know that

$$\dot{C} = 1000\, A \cos 1000\, t.$$

So that, even if M differs only by 1 per cent. from what it

has been assumed to be, that is, if there is only 1 per cent. of magnetic leakage, the neglected term $\left(15 - \frac{M^2}{L'}\right)\dot{C}$ becomes of the value

$$\left\{15 - \frac{(1{\cdot}485)^2}{{\cdot}15}\right\}\dot{C},$$

or $0{\cdot}3\,\dot{C}$, or $300A\cos 1000\,t$. In fact, the neglected term becomes thirty times as important as the important and certainly hitherto non-neglected term RC in the equation.

Now in no case is the current truly a sine function of the time, and any departure from this simplest kind of periodic current makes the error of which I speak much greater.*

* As an example, one of many worked out by my students at Finsbury during the last few years: taking the sizes of iron from a certain Mordey transformer which I have occasionally used; assuming permeability constant and no eddy currents in copper or iron; assuming currents to be true sine functions of the time. If V is voltage at terminals of primary, primary resistance 10 ohms, internal secondary resistance 0·1 ohm, outside resistance of secondary in ohms being called ρ; self-induction of primary 15 secohms, self-induction of secondary 0·15 secohm; assuming V in volts$=1000\sin\frac{2\pi}{\tau}t$, and taking frequency 106 or $\tau=1\div 106$ second, it is quite easy to calculate to any number of places of decimals that may be desired, the amplitudes and lags of the primary and secondary currents, and indeed all other magnitudes involved. The graphic method of working is evidently quite out of the question.

My students have for several years made calculations of this kind, obtaining tables of values for various frequencies and amounts of iron in the transformer, and they are exceedingly instructive. Until such tables are compared with actual experimental results, it seems to me that debates as to the effect of hysteresis consist merely of assertions having no physical basis.

For my present purpose I will give part of two tables calculated by Mr. Eliott, one of my students. Taking the above values:—

1st. If we assume that there is no magnetic leakage. In that case $M=\sqrt{LL'}=1{\cdot}5$ secohms. Using this value of M we get Table I.

2nd. If we assume that there is a little magnetic leakage, say one and one third per cent., or that $M=1{\cdot}48$ secohms. Using this value of M we get Table II.

Now it is perfectly certain that there is some magnetic leakage, always; but it is rather difficult to say just how much there may be. I have here assumed in taking $M=1{\cdot}48$ instead of $1{\cdot}50$ that $1\frac{1}{3}$ per cent. of the total induction due to the primary coils does not pass through the

Of course, any self-induction in the outside part of the secondary circuit will produce the same effect as a leakage in the transformer itself.

The interesting fact to which I wish to draw the attention of members of the Society is this, that however great may be the magnetic leakage, Mr. Blakesley's method is still correct if magnetic permeability is assumed constant during a cycle. Multiplying equation (2) by C we have

$$VC = RC^2 - r\frac{M}{L'}CC' + \frac{LL' - M^2}{L'}C\dot{C}. \quad . \quad . \quad . \quad (3)$$

Integrating for the whole periodic time and dividing by this time—that is, taking the average value of every term in (3) it is to be observed that

$$\frac{1}{\tau}\int_{t}^{\tau+t} C\dot{C}\,.\,dt = \frac{1}{\tau}\int_{C_0}^{C_0} C\,.\,dC = 0,$$

if C_0 is the value of C at the beginning and end of the period; and hence, as $\frac{M}{L'} = \frac{P}{S}$ very nearly,

$$\text{Average } VC = \text{average } RC^2 - \text{average } r\frac{P}{S}CC_1.$$

If either C or C′ were the current in a non-inductive

---

secondary coils, and that $1\frac{1}{3}$ per cent. of the total induction due to the secondary coils does not pass through the primary coils. This number has been taken at random.

The meanings of the letters used at the heads of the various columns are these :—

If $$V = 1000 \sin\frac{2\pi}{\tau}t,$$

$$C = A \sin\left(\frac{2\pi}{\tau}t - \epsilon\right),$$

$$C' = A' \sin\left(\frac{2\pi}{\tau}t - \epsilon'\right),$$

$$V' = a' \sin\left(\frac{2\pi}{\tau}t - \epsilon'\right) = \rho C',$$

P = average power given to primary,
P′ = average power given out by secondary.
Percentage efficiency = 100P′/P.

Evidently V′ is the voltage at the terminals of the secondary circuit. Angles of lag are given in degrees.

circuit, a great error would be introduced by endeavouring to measure the average product by the split dynamometer method; but there is no such error here.

Hence Mr. Blakesley's method is correct, however great may be the magnetic leakage. It must be remembered, however, that I have neglected eddy currents in the copper and iron; and I assume magnetic permeability to be constant *during a cycle*. If I had time, I could show that in alternating-current calculations there are other very important uses of the fact that $\int_t^{\tau+t} x\dot{x}\,.\,dt=0$, if x is any periodic function of the time.

Now at full loads on this transformer it is perfectly obvious that currents, lags, and powers are immensely altered by this small amount of leakage which I have introduced as possible. The currents are ten times as great, and the lags are utterly different from what they have been supposed to be.

TABLE I.—No Magnetic Leakage, or M=1·5.

ρ.	A.	A′.	ϵ.	ϵ'.	P.	P′.	Effic.	a'.
∞	0·1000	0	89·9°	179·9394°	·0850	0	0	100
99 9	0·1412	0·9991	44·97	179·9395	50·06	49·85	99·59	99·82
49·9	0·2232	1·9965	26·50	179·9395	99·95	99·45	99·49	99·61
9·9	0·9956	9·913	0·05	179·9401	497·8	486·4	97·70	99·14
4·9	1·963	19·61	0	179·9409	981·7	942·3	95·98	96 08
0·9	9 09	90·95	0	179·9468	4545	3722	81·70	81·85
0·4	16·67	166·6	0	179·9519	8335	5551	66·61	66·64
0·1	33·33	333·3	0	179·9610	16667	5554	33·33	33·33
0	50	500	0	179·9708	25000	0	0	0

TABLE II.—One and one-third per cent Magnetic Leakage, or M=1·48.

ρ.	A.	A′.	ϵ.	ϵ'.	P.	P′.	Effic.	a'.
∞	·1000	°	89·9°	179·939°	·085	0	0	100
99·9	·1412	0 9858	46·4	181·5	48·61	48·54	99·70	98·49
49 9	·2228	1·968	29·5	182·98	96·96	96·65	99·68	95·98
9·9	·9627	9·458	20·4	194·8	451·2	442·8	98·13	93·63
4·9	1·741	17·15	30·4	204·85	750·8	720·7	96·01	84·05
0·9	3·370	33·28	68·2	247·6	628·4	498·2	79·27	29·95
0·4	3·658	36 11	77·6	257·33	392·8	260·7	66·37	14·44
0·1	3·726	36·78	83·7	263·55	204·4	53·75	26·29	3·678
0	3·739	36·91	85·8	265·7	136·9	0	0	0

Added May 23rd, 1891.

In the discussion of this paper it was obvious that I had not at sufficient length made known what I meant by magnetic "*leakage*." It was owing to this, no doubt, that my introduction of the idea of the importance of leakage was looked upon as academic merely. Again, my use of the symbols L, M, and L′ did not seem to be understood, nor what they had to do with a transformer. It is therefore necessary that I should say more fully, but not more definitely than in the paper, that as M is always less than $\sqrt{LL'}$ owing to magnetic leakage, I define leakage as the value of y where

$$M=(1-y)\sqrt{LL'}.$$

Hence, if $L=P^2\frac{4\pi}{10}\frac{a\mu}{\lambda}$ or P^2m, say, then $L'=S^2m$ and $M=PSm(1-y)$.

Again, the method of treatment to which I object is to state the equations as

$$V=RC+P\frac{dI}{dt}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$O=R'C'+S\frac{dI}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

I affirm that the induction I of equation (1) is a very different thing from the I of equation (2). As the old Maxwell method of writing the equations does not seem to be understood, I wish to make it clear that if I did use the induction I would use I_s in equation (1) and I_s in equation (2), where

$$I_p=PmC+Sm(1-y)C',$$
$$I_s=Pm(1-y)C+SmC'.$$

This is assuming that the number of ampere-turns which produces the effective induction through the primary is $PC+SC'(1-y)$; and the number of ampere-turns which produces the effective induction through the secondary is

$$PC(1-y)+SC'.$$

In fact, as I stated clearly when reading the paper, y is the

fractional portion of the field due to primary current which escapes the secondary winding, or it is the fractional portion of the field due to the secondary current which escapes the primary winding. The reasoning is the same even if a small difference be supposed to exist between the y of the first and the y of the second equation; that is, if M is not the same in the two circuits. I submit that in the absence of any prior definition of "leakage" this is simple and reasonable. Dr. Sumpner's "leakage" is a very different thing. He said, let $I_p = I_s(1+x)$, then x is the magnetic leakage.

Of course, on any reasonable assumption of alterations in μ, the permeability of iron, or on the most reasonable assumption that μ really is constant during a quickly-performed cycle, Dr. Sumpner's x varies greatly during the cycle. I cannot give a physical meaning to x. My "leakage" y does not vary if μ is constant during a cycle, and I have given a perfectly definite physical meaning to it.

It may be well to add, here, why I think it reasonable to assume μ constant during a cycle.

1st. I have shown that if there is any leakage, the use of equations (1) and (2) is quite unreasonable.

2nd. If there is any leakage, it is impossible to treat the subject mathematically unless some assumption is made concerning μ.

3rd. It is quite certain that the law of variation of μ when the cycle is very slowly performed must be quite different from the law when the cycle is very quickly performed, as it always is in practice.

4th. The analogies between magnetic stress and strain and ordinary stress and strain in materials are well established. Now every material exhibits strain hysteresis when slowly loaded and unloaded, and exhibits no hysteresis whatever when the loading and unloading are very quickly performed. Even the most inelastic of materials will transmit a musical note unchanged. Hence for years I have taught my students to look upon magnetic hysteresis as very important when cycles are slowly performed and as unimportant when cycles are very quickly performed. Unless on this assumption, how is it that there is so little heating of the iron of a transformer by hysteresis even when transforming the largest amounts of

energy; and such heating as there is must be partly due to eddy currents. I therefore maintain that μ constant during a cycle (and this means that my "leakage" is constant during a cycle) is the only reasonable assumption that can be made in the present state of our knowledge.

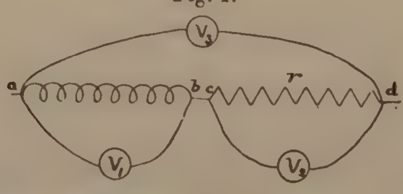
But even if this reasonable assumption of no hysteresis and of the constancy of μ during a cycle be denied me, and if I must assume the possibility of its being wrong, still $\frac{LL'-M^2}{L'}$ must be more nearly constant than μ; for it is equal to $2Ly$. And if μ increases and therefore L increases, y will certainly diminish, and if L diminishes y will certainly increase.

XXIV. *Alternate Current and Potential Difference Analogies in the Methods of Measuring Power.* *By* Prof. W. E. Ayrton, *F.R.S., and* W. E. Sumpner, *D.Sc.**

I.

In a paper read by us before this Society on March 6th it was pointed out that for every problem involving alternate *P.Ds. in series* there was an analogous problem involving alternate *currents in parallel*. This general proposition tells us that we can transform each of the P.D. equations given, for example, in our paper on "The Measurement of the Power given by any Electric Current to any Circuit," read before the Royal Society, April 9th, 1891, into a current equation,

Fig. 1.

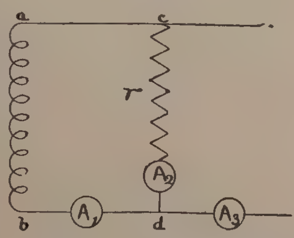


and so transform our method of calculating power by the measurement of three P.Ds. into a method of calculating power by the measurement of three currents.

* Read June 12, 1891.

Such a transformation of our equations has been recently carried out by Dr. Fleming in the 'Electrician,' for May 8th, and the method he arrives at as well as the three-voltmeter method of which it is an analogue are seen in figures 2 and 1. If V_1, V_2, V_3 be the readings of the voltmeters in figure 1, and A_1, A_2, A_3 the readings of the three ammeters in figure 2, and r the resistance of the non-inductive portion of the circuit cd in each case, then the mean watts given to ab are respectively, whatever be the nature of the circuit ab, or of the current

Fig. 2.



$$\frac{1}{2r}(V_3^2 - V_1^2 - V_2^2)$$

and

$$\frac{r}{2}(A_3^2 - A_1^2 - A_2^2).$$

The three-ammeter method has the advantage over the three-voltmeter method, in that the dynamo need not give a larger P.D. than that necessary to send the current through ab; it is inferior to the three-voltmeter method in that while it is possible to measure V_1, V_2, and V_3 rapidly in succession by using only one voltmeter, it is, of course, impossible to use only one ammeter to measure A_1, A_2, and A_3 without constantly interrupting the circuit, and hence it would be necessary to accurately calibrate three instruments if this current method were employed.

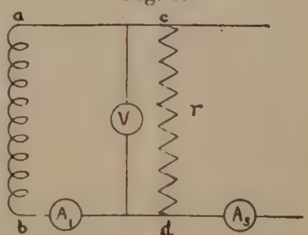
But the main objection to this current method is that, as Dr. Fleming points out, it does not possess the accuracy of our three-voltmeter method of measuring power. For in order that this three-current method may give accurate results it is necessary to assume, to quote from our Royal Society paper, "the entire absence of self and mutual induction from a circuit some portion of which is necessarily of a solenoidal form."

It is possible, however, to obtain a current analogue of our three-voltmeter method which shall have the accuracy of the

three-voltmeter method itself. And as the general proposition given in our previous paper tells us that the current analogues of P.D. arrangements *in series* are current arrangements *in parallel*, it follows that with this other method also the dynamo will not be required to produce a greater P.D. than that necessary to send the current through the circuit the power given to which we desire to measure.

The method is as follows :—In parallel with the circuit *ab* (fig. 3) the power given to which we wish to measure connect a non-inductive resistance of *r* ohms (in circuit with which no instrument is placed, which would necessarily make the so-called non-inductive branch more or less inductive). Let A_3, A_1, and V be the readings of the two ammeters and the voltmeter placed as shown, then, from the equations given in our Royal Society paper, it follows at oncethat the mean watts given to *ab* are

Fig. 3.



$$\frac{r}{2}\left\{A_3^2 - A_1^2 - \left(\frac{V}{r}\right)^2\right\}.$$

It is interesting to notice that if *ab* were the primary coil of a transformer, it would be when the load on the secondary was small, that is when the current passing through *ab* was small, that it would be most difficult, on account of lag, to measure with ordinary methods the power given to *ab*. But that is exactly the case when it is most easy to use our one-voltmeter and two-ammeter method, since when the dynamo has to send little current through *ab* there is little objection to requiring it to send a current through *cd* in parallel with *ab*.

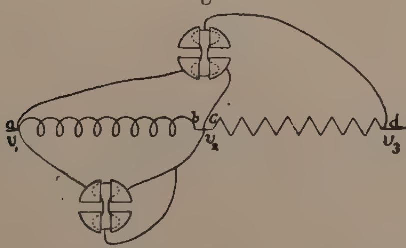
If the voltmeter (fig. 3) be a hot wire instrument, then, since an appreciable current will pass through this voltmeter, *r* must be taken as the parallel resistance of *cd* and of the voltmeter. It is important to observe, however, that there is no necessity to know either of these resistances separately, since the value of *r* can be determined when the three instru-

ments A_1, A_3, and V are relatively calibrated thus:—First, break the circuits of *cd* and of the voltmeter, and compare the deflexion of A_3 with A_1; this calibrates ammeter A_3 relatively to ammeter A_1, the calibration of which we will assume to be correct; second, close the circuits of *cd* and of the voltmeter, but break the transformer circuit *ab*, A_3 is now in series with the parallel circuit containing *cd* and the voltmeter. The value of r is therefore at once known, since r must equal the volts as read by V divided by the amperes as read by A_3.

II.

As an illustration of the general proposition to which we have referred, it may interest the Members to see what are the other analogies that we have traced out between alternate P.Ds. in series and alternate currents in parallel in connexion with the measurement of power.

Fig. 4.



Mr. Blakesley's method, communicated to this Society in February of this year *, is the current analogue of our electrometer method of 1881. For with the electrometer method (fig. 4) we make two measurements, one giving us the mean value of

$$(v_1-v_2)\left(v_3-\frac{v_1+v_2}{2}\right),$$

the other the mean value of

$$\frac{(v_1-v_2)^2}{2},$$

v_1, v_2, v_3 being the instantaneous values of the potentials.

* *Ante*, p. 106.

Then we take the difference, and so get the mean value of

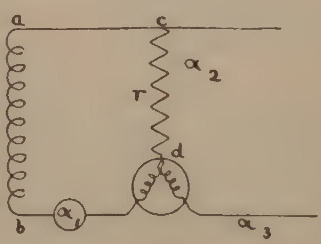
$$(v_1 - v_2)(v_2 - v_3),$$

which is equal to r times the mean watts given to ab (fig. 4).

With Mr. Blakesley's method two measurements are made, one with a split dynamometer (fig. 5) giving the mean value of

$$\alpha_1 \alpha_3,$$

Fig. 5.



and the other with an ammeter giving the mean value of

$$\alpha_1^2,$$

α_1, α_2, α_3 being the instantaneous values of the currents. Then the difference is taken, and so the mean value of

$$\alpha_1(\alpha_3 - \alpha_1) \text{ or } \alpha_1 \alpha_2$$

is obtained, and this is equal to $\frac{1}{r}$ times the mean watts given to ab (fig. 5).

It is important to notice that as no instrument is inserted in the non-inductive circuit ab (fig. 5), this method of Mr. Blakesley's has exactly the same accuracy as the electrometer method.

III.

The electrometer measurements may, as we pointed out some years ago, be varied; and by making the connexions as seen in fig. 6, we can obtain from the two readings the mean values of

$$(v_2 - v_3)\left(v_1 - \frac{v_2 + v_3}{2}\right)$$

and

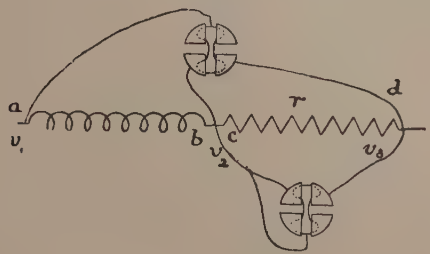
$$\frac{(v_2 - v_3)^2}{2};$$

then, taking the difference, we get the mean value of

$$(v_2 - v_3)(v_1 - v_2),$$

which, as before, is equal to r times the mean watts given to *ab*.

Fig. 6.



Mr. Blakesley has also pointed out, in his paper read in February of this year (*ante*, p. 106), that the current measurements may be varied and the apparatus arranged as seen in fig. 7. The two measurements now give respectively the mean values of

$$\alpha_2\alpha_3$$

and

$$\alpha_2^2;$$

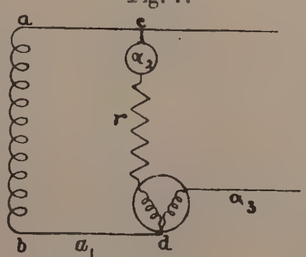
therefore the difference gives the mean value of

$$\alpha_2(\alpha_3-\alpha_2),$$

that is, the mean value of

$$\alpha_2\alpha_1,$$

Fig. 7.



which is equal to $\frac{1}{r}$ times the mean watts given to *ab*.

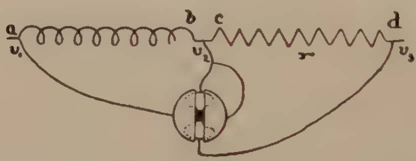
While, however, our second method of using the electrometer (fig. 6) gives the answer with the same accuracy as the first method (fig. 4), Mr. Blakesley's second method of joining up the dynamometers in figure 7 introduces self-induction into a circuit which ought to be entirely non-inductive, and so it does not give the answer with the same accuracy as his first method (fig. 5).

IV.

The modification of our electrometer method suggested to one of us by Mr. L. Atkinson, while he was a pupil at the

Finsbury Technical College, and afterwards carried out by MM. Blondlot and Curie, of making an electrometer with two needles for the measurement of power, is the exact analogue of the wattmeter method. For with the double-needle electrometer (fig. 8) we obtain from a single reading

Fig. 8.



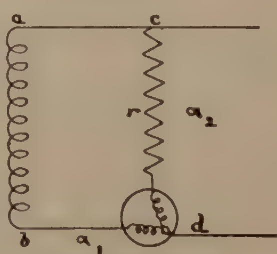
the mean value of

$$(v_1-v_2)(v_2-v_3),$$

which is equal to r into the mean watts given to ab. Similarly with the wattmeter (fig. 9) we obtain from a single reading the mean value of

$$a_1 a_3,$$

Fig. 9.



which is proportional to $\frac{1}{r}$ into the mean watts given to ab.

While, however, the double-needle electrometer gives us the answer with perfect accuracy, the wattmeter method is liable to inaccuracy from the circuit cd not being strictly non-inductive.

Some years ago, at a meeting of the Institution of Electrical Engineers, one of us published the formula for the error made in using a wattmeter to measure the power given by an alternate current to an inductive circuit. Not wishing to cumber the pages of scientific periodicals with elementary mathematics, it was thought sufficient merely to state this formula without publishing a proof. But as our formula has now been introduced into text-books, and as the appropriation thereof by the writer of a well-known treatise has led him to supply a proof of it involving an appalling

array of mathematical equations, we venture to offer a proof which, although very simple, is perfectly accurate. We are the more induced to do this because we find that this formula, and its proof for the error due to self-induction in the supposed non-inductive portion of the circuit *cd*, apply equally well to all the nine methods of measuring power illustrated in figures 1 to 9 of this paper.

The formula employed with such methods for giving the mean watts, whether it involves the reading of one instrument, as in the case of the wattmeter (fig. 9), or of two instruments, as with the methods illustrated in figures 4, 5, 6, 7, and 8, or of three instruments, as with the methods illustrated in figures 1, 2, and 3, gives with perfect accuracy r times the mean product of two currents, or $\frac{1}{r}$ times the mean product of two P.D.s. Whether this mean product is directly proportional to the mean watts given to *ab* depends in all the nine cases on the following consideration :—

The mean product between two currents which are sine functions of the time is, as every student now knows, equal to half the product of their maximum values into the cosine of the phase angle between them. Therefore if the angle of lag between the current in *ab* and the P.D. between its terminals be θ, and the angle of lag between the current in *cd* and the P.D. between its terminals be ϕ, and if the maximum values of the currents in these two circuits be A_1' and A_2' respectively, and the maximum values of the P.D.s at the terminals of these circuits be V_1' and V_2', it follows that the formula used to measure the watts in the cases 2, 3, 5, 7, and 9 gives

$$r\frac{A_1'A_2'\cos(\theta-\phi)}{2},$$

and in the cases 1, 4, 6, and 8,

$$\frac{V_1'V_2'\cos(\theta-\phi)}{2r}.$$

But what we want to measure is the mean product of the

current in *ab* into the P.D. between its terminals, and this product equals

$$\frac{A_1'V_1' \cos\theta}{2}.$$

But

$$rA_2' = V_2' \cos\phi;$$

and in the methods illustrated in the figures 2, 3, 5, 7, and 9

$$V_2' = V_1',$$

while in the methods illustrated in the figures 1, 4, 6, and 8

$$A_2' = A_1';$$

therefore in all the nine cases

$$\frac{\text{Apparent watts}}{\text{True watts}} = \frac{\cos(\theta - \phi) \, . \cos\phi}{\cos\theta}$$

$$= \frac{1 + \tan\theta \, . \tan\phi}{1 + \tan^2\phi} \quad . \quad . \quad . \quad . \quad (1)$$

The circuit *cd* need only possess a self-induction *l*, even if an ammeter or dynamometer form part of it, but the circuit *ab* may have mutual induction and capacity as well as self-induction. If therefore we write expression (1) in the form

$$\frac{1 + \frac{Lp}{R} \cdot \frac{lp}{r}}{1 + \left(\frac{lp}{r}\right)^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

as we did in 1888, where *p* equals 2π times the frequency, it must be remembered that while *l* and *r* are the true values of the self-induction and resistance of *cd*, L and R are only the *effective* self-induction and resistance of *ab*. Hence, as Mr. Blathy suggested in the 'Electrician' for 1888, it is better to leave our expression for the ratio of the apparent to the true watts in the general form as given in (1) rather than to put it in the derived form as given in (2).

ϕ will generally be positive if the resistance of *cd* is small; but, if *cd* contains a doubly-wound high resistance-coil, as is generally the case when *cd* is the fine-wire circuit of a wattmeter, then it is quite possible to make ϕ positive, nought, or negative. θ may, of course, be also positive, nought, or

negative, depending on whether the self and mutual induction effects preponderate or not over the capacity effect. It is therefore possible to have either θ or ϕ, or both, positive or negative.

The apparent watts will therefore be :—

too large if θ and ϕ be both of the same sign and $\theta > \phi$;

too small if $\begin{cases} (1) & \theta \text{ and } \phi \text{ be both of the same sign and } \theta < \phi; \\ (2) & \theta \text{ or } \phi \text{ be of different signs ;} \end{cases}$

correct if $\begin{cases} (1) & \theta \text{ and } \phi \text{ be equal ;} \\ (2) & \phi \text{ be nought.} \end{cases}$

Now ϕ can be made very small in one or other of three ways :—

1. Use some method of testing, like that shown in figures 1, 3, 4, 5, 6, and 8, which does not require any measuring instrument to be placed in the non-inductive circuit *cd*.
2. Use a wattmeter (fig. 9), and make the capacity of the stationary doubly-wound resistance-coil exactly balance the self-induction of the suspended and the stationary coils.
3. Make the resistance of the fine-wire circuit, *cd*, of the wattmeter small. For with a given P.D. between the terminals of *cd* the same deflexion of the measuring instrument can be obtained for different values of the resistance of *cd* if we make the number of turns in the coil or coils of the measuring instrument in *cd* proportional to the resistance *cd*. But the self-induction of the coil or coils is proportional to the square of the number of turns, and therefore proportional to the square of r for a given deflexion of the measuring instrument. Hence tan ϕ can be made as small as we like for a given value of p by making the resistance of *cd* small.

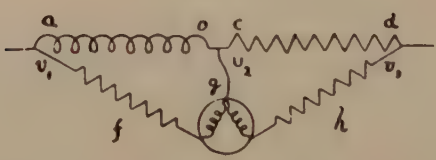
This suggests a current method of measuring the power given to any circuit which is no more wasteful of power than the methods shown in figures 2, 3, 5, and 7 ; and which, although not so accurate as those shown in figures 3 and 5, is as accurate as those shown in figures 2 and 7. The method

is simply to use a wattmeter (fig. 9), but having both its coils made of thick wire, or, as this may be called a split dynamometer, the method consists in using a split dynamometer having one of its coils in the circuit *ab* (the power given to which we desire to measure) and the other coil in a circuit *cd* parallel to *ab*. The power will be given at once by *r* times the reading of the instrument and with but a very small error if *r* be small.

V.

Mr. Rimington has suggested a method of measuring the mean value of the product $(v_1 - v_2)(v_2 - v_3)$ (fig. 9) by means of a dynamometer, each of whose coils is in circuit with a high resistance, joined up as shown in figure 10.

Fig. 10.



The objection to this method is as follows:—By making the time-constants of each of the circuits of the dynamometer *a f g*, *d h g* equal to one another, we can, no doubt, make the difference of phase in the two currents passing through the dynamometer exactly the same as the difference in phase between the current through *ab* and the P.D. at the terminals of *ab*; but we cannot make the currents through the dynamometer coils independent of the rate of alternation. Hence, if this instrument be employed for measuring the power given to *ab* in the way shown in figure 10, it must be calibrated for each rate of alternation of the current.

But although this defect exists in the employment of Mr. Rimington's high-resistance split dynamometer for the measurement of power, it can be used without error for measuring the phase-angle between *two P.D.s in series* by a method analogous to that employed by Mr. Blakesley for measuring the phase-difference between *two currents in parallel*.

Fig. 11 shows Mr. Blakesley's method: the dynamo-

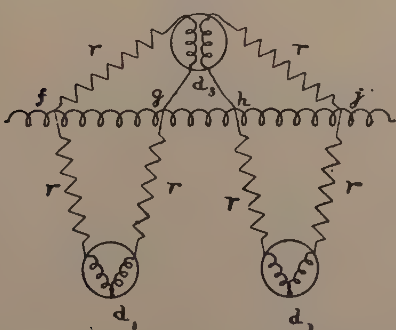
Fig. 11.

meters 1 and 2 give respectively the mean squares of the currents in the two circuits, and the dynamometer 3 the mean product of the two currents; and from the three readings we have, as is now well known,

$$\cos\theta = \sqrt{\frac{\text{Square of reading of 3}}{\text{Reading of 1} \times \text{Reading of 2}}},$$

where θ is the angle of lag.

Fig. 12.



Now let fg and hj be two circuits in series (fig. 12), and let it be required to find the angle of phase-difference θ between the P.D. at the terminals of fg and the P.D. at the terminals of hj. Connect up the high-resistance dynamometer successively as shown, and let d_1, d_2, and d_3 be the three deflexions obtained.

Each circuit of the dynamometer consists of a coil of fine wire, and a non-inductive high resistance in series with it. Let r be the total resistance of each circuit, and let ϕ be the angle of lag between a current in either circuit of the dynamometer and the P.D. at its terminals.

Let V_1' and V_2' be the maximum values of the P.D. between f and g, and between h and j respectively, then

$$d_1 \propto \tfrac{1}{2}\left(\frac{V_1'}{2r}\right)^2 \cos^2\phi,$$

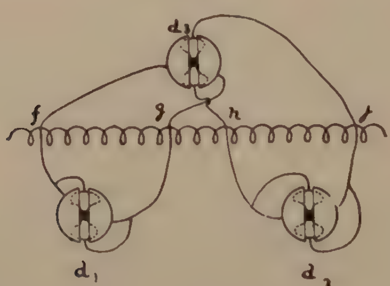
$$d_2 \propto \tfrac{1}{2}\left(\frac{V_2'}{2r}\right)^2 \cos^2\phi,.$$

$$d_3 \propto \tfrac{1}{2}\left(\frac{V_1'}{r}\cos\phi \times \frac{V_2'}{r}\cos\phi\right)\cos\theta,$$

$$\therefore \quad \cos\theta = \sqrt{\frac{d_3^2}{16\, d_1 d_2}}.$$

Figure 13 shows the way in which the Blondlot and Curie double-needle electrometer can be successively connected up

Fig. 13.



so as to obtain, from the readings d_1, d_2, d_3, the angle of phase-difference between the P.D. at the terminals of $f\,g$, and the P.D. at the terminals of $h\,j$, the formula being, of course,

$$\cos\theta = \sqrt{\frac{d_3^2}{d_1 \times d_2}}.$$

If we desire to measure the angle of lag between the current in any circuit ab and the P.D. between its terminals, we can employ either the three-voltmeter method (fig. 1) or either of its analogues, viz. the three-ammeter method (fig. 2), or the one-voltmeter and two-ammeter method (fig. 3).

Fig. 1.

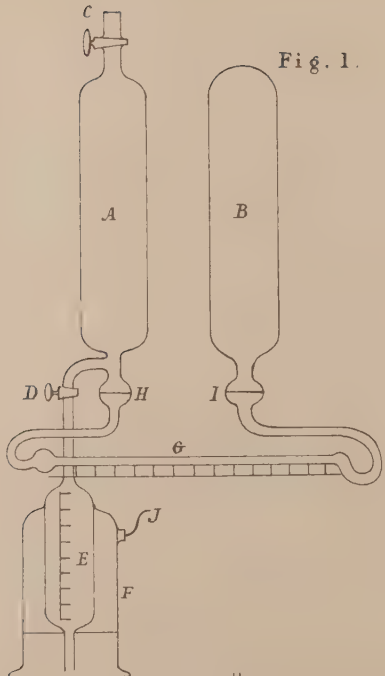


Fig. 2.

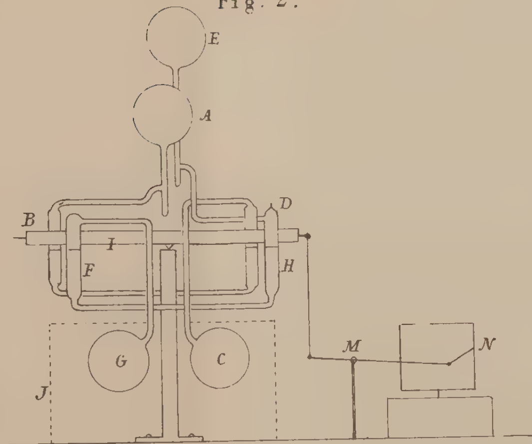


Fig. 3.

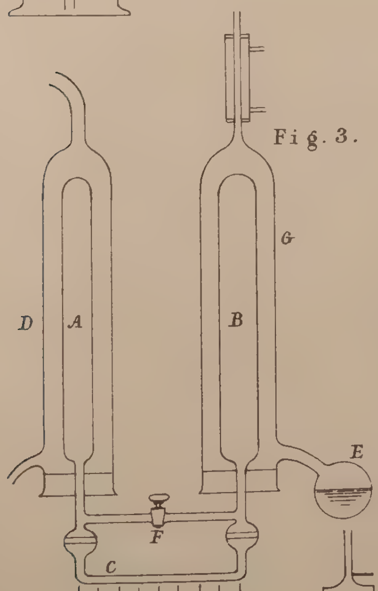


Fig. 4.

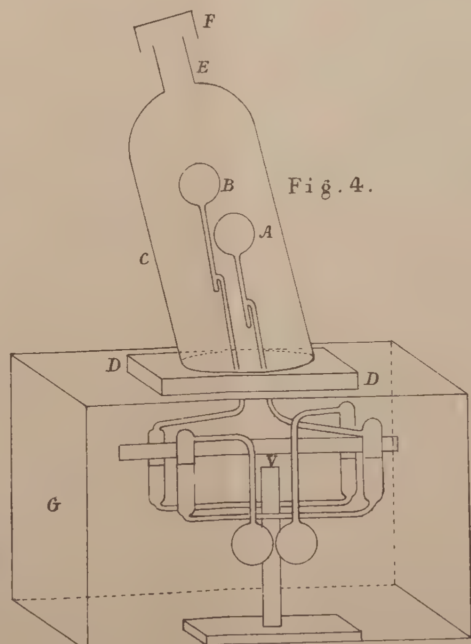


Fig. 5.

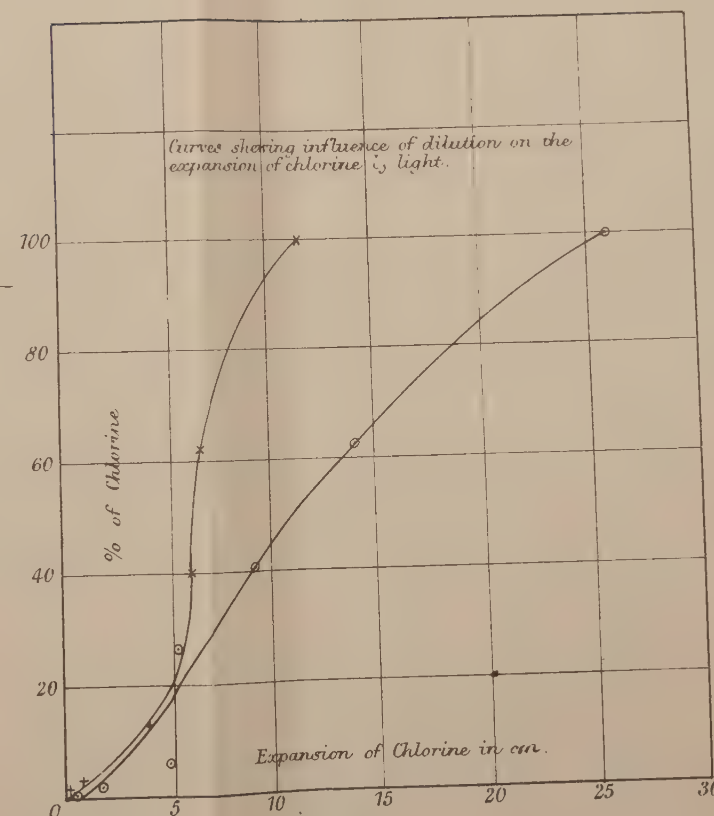


Fig. 1.

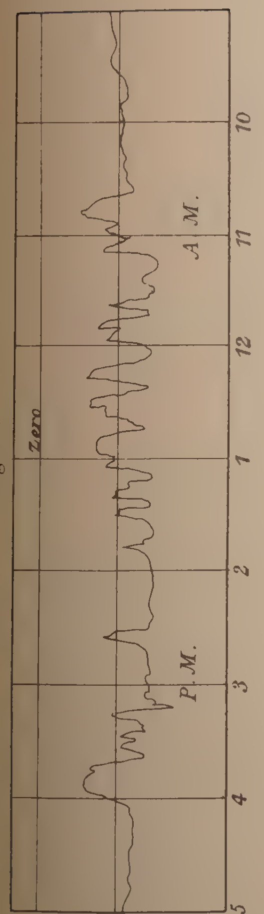


Fig. 2.

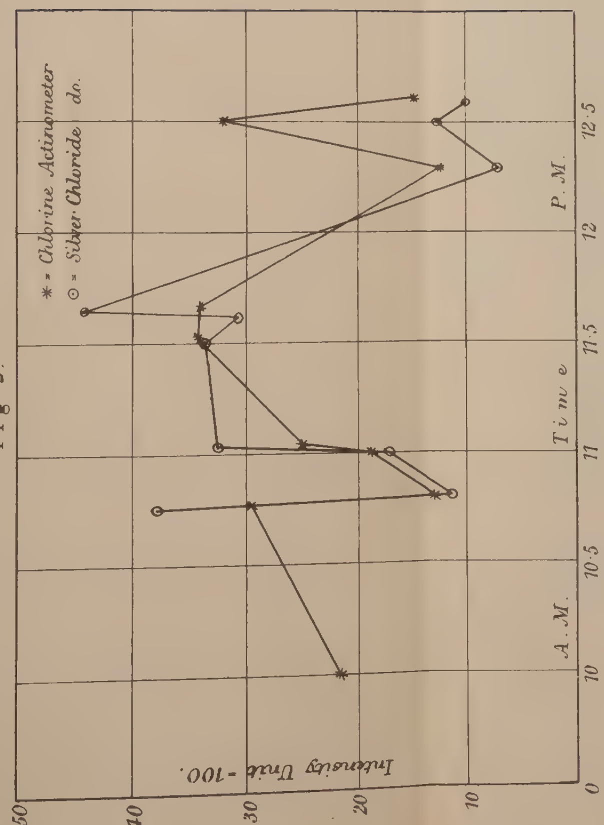
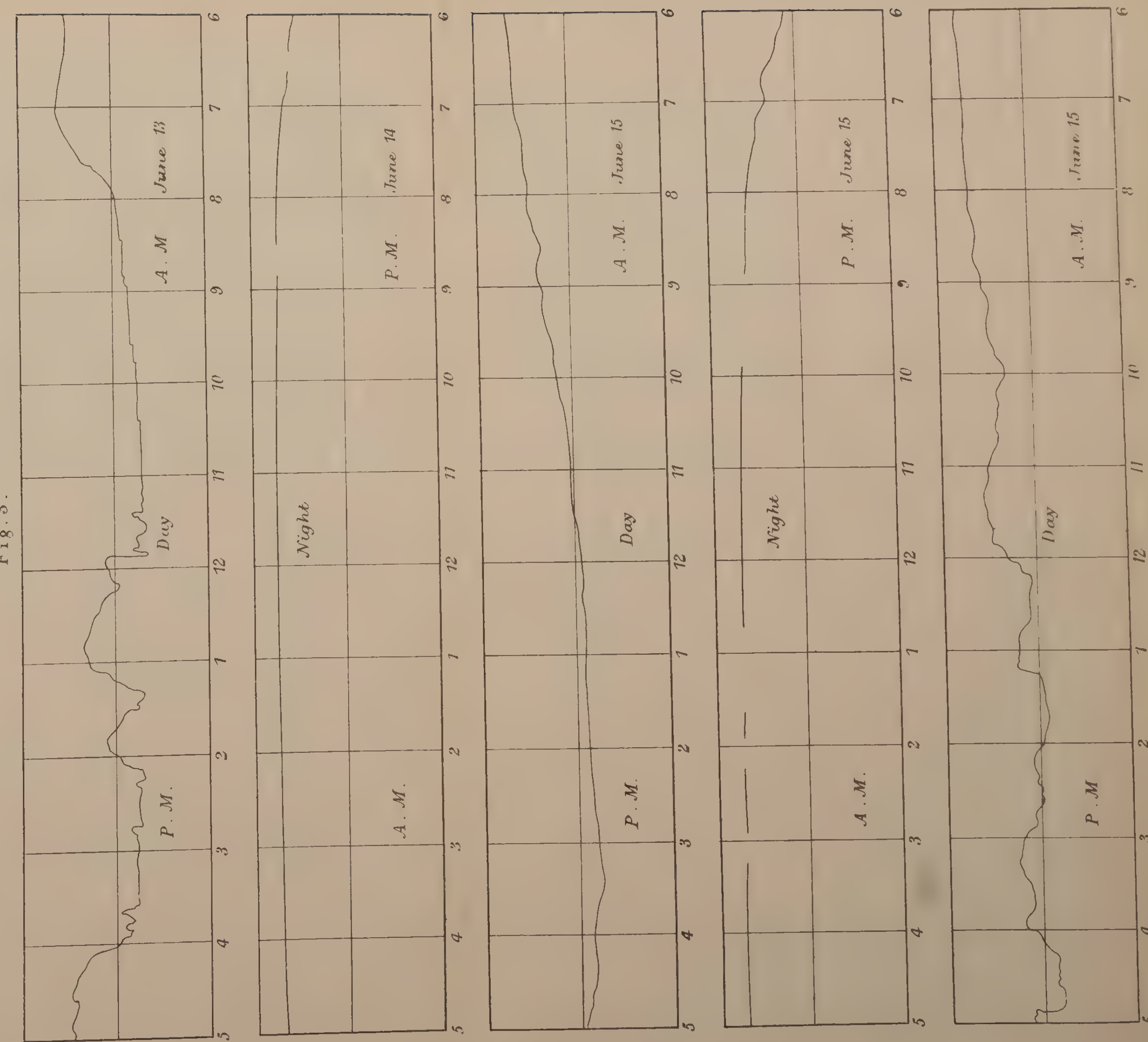


Fig. 3.



The formulæ giving the cosine of the lag angle for the three methods are, respectively,

$$\cos\theta = \frac{V_3^2 - V_1^2 - V_2^2}{2V_1V_2},$$

$$\cos\theta = \frac{A_3^2 - A_1^2 - A_2^2}{2A_1A_2},$$

$$\cos\theta = \frac{A_3^2 - A_1^2 - \left(\frac{V}{r}\right)^2}{2A_1\frac{V}{r}},$$

V_1, V_2, V_3, A_1, A_2, A_3, and V being the readings of the instruments in the different cases.

XXV. *The Expansion of Chlorine by Light as applied to the Measurement of the Intensity of Rays of High Refrangibility.* *By* Dr. A. RICHARDSON, *Lecturer on Chemistry, University College, Bristol**.

[Plates III. & IV.]

IT has been shown by Budde (Phil. Mag. iv. 1871; Pogg. *Ann.* Ergbd. vi. 1873) that when chlorine is exposed to the influence of sunlight, an expansion of the gas occurs which is independent of the direct heating-effects due to the light; the volume to which the gas first expands is maintained during exposure provided that the intensity of the light remains constant, contraction to the original volume taking place when the gas is shaded. He further found that the rays of high refrangibility were influential in promoting this change, no expansion being occasioned by the rays at the red end of the spectrum. The application of this property of

* Read June 26, 1891.

chlorine to the measurement of the "actinic"* intensity of light was suggested by Budde many years ago, but no further steps appear to have been taken in this direction.

Some experiments on which I am at present engaged have rendered it necessary that the actinic intensity of light should be measured during periods of many months together, and it seemed possible that the expansion of chlorine by light might be applied to this purpose. As, however, the researches of Bunsen and Roscoe (Trans. Roy. Soc. 1887, p. 381) led them to the conclusion that no change in volume occurred in chlorine, when exposed to light, other than that due to direct heating-effects, it became necessary to repeat some of Budde's experiments so as if possible to decide this point. In order to do this a differential apparatus was taken, consisting of two bulbs of 160 cubic centim. capacity each and connected by a capillary tube containing strong sulphuric acid; any change in pressure of the gas in either bulb was indicated by the movement of an index of air contained in the capillary.

The bulbs were first filled with dry air and placed in a glass tank through which a current of cold water circulated; under these conditions it was found that no movement of the index took place on exposure to bright sunlight. The air in one of the bulbs was then replaced by dry chlorine, and both bulbs were again exposed under precisely similar conditions; an immediate expansion of the chlorine took place, causing the index to move through a distance of from 20 to 30 centim., but on shading the bulbs the index returned to zero. On interposing variously coloured glasses between the bulbs and the source of light, it was found that with cobalt glass the index receded 15 to 20 centim. from the zero-point, whilst even faintly yellow glass as well as ruby glass caused it to return to zero. It was further found that the radiations from a cannon-ball at a temperature just below a red heat produced practically no movement of the index when the bulbs were exposed in the air at a distance of from 1 to ·5 metre.

Budde's observations being thus confirmed, it was next necessary to determine how far the expansion of chlorine is

* The term "actinic" is used for brevity to denote rays at the violet end of the spectrum.

proportional to the actinic intensity of the light to which it is exposed. In order to do this a series of measurements of the expansion of the gas were made, whilst at the same time the intensity of the light to which the gas was exposed was determined by means of an actinometer.

The expansion of the gas was measured in a differential arrangement, consisting of two tubes of 55 cubic centim. capacity and 10 centim. in length; these were connected with a horizontal gauge graduated in ·5 centim., provided with a small bulb at each end. These bulbs and also the gauge contained strong sulphuric acid, a short column of air being introduced to serve as index.

The tubes to be exposed were suspended in a box, which could be placed at any required angle so as to face the sun, and when filled with dry air were found to be equally heated; the bulbs containing the acid were in all cases protected from the light. One tube was then filled with dry chlorine and the acid up to the index was saturated with the gas.

The chlorine used in this and subsequent experiments was prepared by the action of hydrochloric acid on potassium bichromate; traces of hydrochloric acid were removed by passing the gas through U-tubes containing solutions of chromic acid, and subsequently dried by means of sulphuric acid. In order to determine the actinic intensity of the light, a modified form of Bunsen and Roscoe's pendulum-actinometer was used (Trans. Roy. Soc. 1862, p. 139); in this method paper coated with silver chloride is exposed to light, and the time required to produce a degree of darkening equal to a standard tint is measured. The intensity of light necessary to produce this tint in one second of time is taken as the unit of intensity. The measurements were made in the open air, care being taken that the air- and chlorine-tubes were exposed to light in the same plane as the sensitive paper. When the maximum expansion of the chlorine was reached at any one time, as shown by the index remaining stationary, the intensity of the light was measured by exposing the sensitive paper. Two series of observations were made under widely different conditions of light; one on June 8, which was a cloudy day, the other on June 9, on which the sun was

shining. The results of these measurements are given in the following table:—

First Series.				
Time.	I. Intensity of light (unit =1).	II. Expansion of Cl, expressed in centimetres on scale.	III. Ratio $\frac{I}{II}$.	IV. Recalculation of intensities from observed expansion.
m. s.				
2 50	0·0242	1·95	0·0124	0·0263
2 55	0·0571	3·20	0·0178	0·0432
3 0	0·0461	3·20	0·0144	0·0432
3 5	0·0291	1·87	0·0155	0·0252
3 15	0·0196	1·40	0·0140	0·0188
4 0	0·0382	2·70	0·0141	0·0364
Second Series.				
10 45	0·3758	28·15	0·0133	0·3799
10 50	0·1022	7·95	0·0128	0·1073
11 0	0·1693	12·2	0·0138	0·1647
11 5	0·3240	27·95	0·0116	0·3773
11 15	0·4381	26·20 ?	0·0167	0·3537
11 30	0·3348	28·0	0·0119	0·3779
11 35	0·3212	29·95	0·0107	0·4042
11 36	0·4457	30·80	0·0144	0·4158
12 20	0·0779	6·15	0·0126	0·0890
12 30	0·1210	7·70	0·0157	0·1040
12 32	0·1087	7·65	0·0142	0·1032
12 35	0·1133	9·25	0·0122	0·1249
4 0	0·1108	10·75	0·0103	0·1451
4 5	0·1141	10·05	0·0113	0·1357
4 15	0·1150	7·75	0·0148	0·1046

In column I. the intensity of the light is given as measured by the actinometer. In column II. the expansion of the chlorine, as shown by the movement of the index from zero, is given in centimetres; the zero-point, which was determined from time to time by shading the tubes, was found to remain nearly stationary throughout. The calculated intensity, in terms of the movement of the index through 1 centim., is given in column III., and from the mean value so obtained the intensity is recalculated in terms of the observed expansion (column IV.). In making these observations great difficulty was experienced owing to the constant variation in the intensity of the light; and as the maximum movement

of the index was only reached after some time, these sudden variations, which were registered accurately by the actinometer, did not produce a corresponding change in the position of the index. The close agreement between the results obtained directly by the actinometer and those calculated from the expansion of the gas lead, however, to the conclusion that the change in volume in chlorine gas is directly proportional to the actinic intensity of the light to which it is exposed, although the friction of the acid in the gauge tends to smooth out the more sudden variations.

By the aid of such a differential apparatus as that above described, to which the name of "chlorine actinometer" may be applied, the intensity of the light can be read directly from the gauge, and it is thus found possible to study other light-effects under constant conditions of intensity by making exposures whenever the index stands at a given point.

It was next necessary to inquire how far dilution with air influenced the amount of expansion of chlorine when exposed to light. To do this a differential apparatus, shown in fig. 1, Plate III., was used, in which the gas to be exposed was contained in the tubes A and B, 15 centim. in length and about 3 centim. in diameter. Two glass stop-cocks, C and D, were sealed on to A, the lower one, D, being connected with a graduated pipette E containing air and dipping under strong sulphuric acid contained in F. The graduated capillary tube G was filled with strong sulphuric acid, as also the bulbs H and I, the movement of a short column of air serving to indicate any change in volume in either tube. The tube A was first filled with chlorine, whilst B contained dry air throughout the experiment. Both tubes were then exposed to light and the expansion of the chlorine determined, the intensity of the light being at the same time noted by means of the chlorine actinometer; the tubes were then shaded and the zero-point found. The stop-cocks C and D were then opened, and by blowing air in at the side tube J a measured volume of acid was caused to rise in E, thus forcing an equal volume of air into A. Chlorine being expelled at C; C and D were then closed, and, after the gas had thoroughly mixed, the tubes were again exposed to light of the same intensity as in the first case: this process was repeated until all the

chlorine was replaced by air. Knowing the capacity of A and the proportion of air introduced at each operation, the amount of chlorine could be calculated. Two series of experiments were made under different conditions of light, the intensity for each series remaining nearly constant. The results are given in the next table.

First Series.

No.	Expansion of Cl in centim.	Per cent. of Cl in gas.	Expansion of Cl in actinometer.	Corresponding intensity of light (unit = 1).
1.	26·5	100	8·0	·1107
2.	14·5	63·7	7·9	·1092
3.	9·25	41·0	17·9	·1092
4.	5·25	26·1	17·9	·1092
5.	4·75	7·1	7·9	·1092
6.	2·0	2·0	7·9	·1092
7.	0·5	0·5	7·95	·1100
8.	0·0	0·1	7·9	·1092

Second Series.

No.	Expansion of Cl in centim.	Per cent. of Cl in gas.	Expansion of Cl in actinometer.	Corresponding intensity of light (unit = 1).
1.	11·7	100	3·55	·0491
2.	6·25	63·7	3·5	·0484
3.	5·95	41·0	3·5	·0484
4.	4·35	11·3	3·3	·0456
5.	1·0	3·0	2·2	·0304
6.	0·15	0·8	3·4	·0466

It is seen from the double nature of the curve, fig. 5, Plate III., that the first effect of dilution is very marked, diminishing after a time, and finally increasing when only a small per cent. of chlorine remains in the tube. It was found, however, that chlorine was gradually given off (from the sulphuric acid which was saturated with the gas) to such an extent that, after complete expulsion of the chlorine by air, on again exposing the tube to light, after two days, an expansion of 5 centim. occurred. This is a point of some importance where the expansion of chlorine as against air is made a measure of the actinic intensity of light, as the

diffusion of chlorine from the sulphuric acid into the air-bulb would introduce a gradually increasing source of error *.

As it seemed possible that the sensitiveness of chlorine to light might be influenced by the temperature of the gas during exposure, an experiment was made in which the two tubes A and B, fig. 2, Plate III., were filled with chlorine, these were connected together by a tube which could be closed by means of a stop-cock F; changes in volume being measured as before by means of the gauge C containing sulphuric acid and bubbles of air. The tube A was kept at a temperature of 14° C. by a current of water circulating through the outer tube D, whilst B was heated in the jacketing tube G G, by the vapour from chlorobenzene boiling in E at a temperature of 132° C. The pressure in the two tubes was at first equalized by opening the stop-cock F; and when the temperature was constant in the two tubes (as shown by the index remaining stationary when F was closed), the tubes were alternately exposed to and shaded from the light. The following is the mean of a series of experiments made:—

Both tubes shaded, zero	=	0.
Heated tubes exposed	=	6·0 centim. expansion.
Both tubes shaded, zero	=	0.
Cooled tubes exposed	=	6·7 centim. expansion.
Both tubes shaded, zero	=	0.

In the heated tube the illumination of the gas was interfered with by the partial refraction of the light, due to the condensation of the chlorobenzene on the sides of the jacketing tube, and further it is possible that the vapour of chlorobenzene may absorb a portion of the actinic rays. This probably accounted for the slight difference observed in the expansion of the gas in the two tubes. From this experiment it is concluded that the expansion of chlorine by light is practically unaffected within a range of temperature between 14° and 138° C.

Having made these preliminary experiments, an apparatus was next devised whereby the automatic registration of the

* In the chlorine actinometer it is found desirable to cut off communication between the two bulbs, when not in use, by means of a stop-cock.

actinic intensity of light was effected by the expansion of chlorine. This was done by suspending a differential apparatus on the beam of a balance in such a manner that the flow of acid from one arm to the other produced a movement of the beam, which was communicated by means of a lever to a pen and was recorded on a rotating drum.

As has already been stated, a differential actinometer, containing air in one bulb and chlorine in the other, gradually becomes inaccurate owing to the diffusion of chlorine into the air-bulb. This difficulty is avoided in the apparatus shown in fig. 3, Plate III., which consists of two differential arrangements A B C D and E F G H, suspended on the beam of the balance I; one is completely filled with dry chlorine, the tubes B and D being half filled with strong sulphuric acid saturated with the gas. The second contains dry air with sulphuric acid in F and H. One of the chlorine-bulbs A and one of the air-bulbs E are exposed to light, whilst the other chlorine-bulb C and air-bulb G are protected from the light by a covering of tin-foil, and hang in the box J, in which the two bulbs can swing freely. It will be seen that when A and E are exposed to the heating-effects of sunlight, the expansion of the gas in A causes acid to flow from B to D, whilst a corresponding expansion in E causes an equal weight of acid to flow from H to F. But the chlorine in A undergoes a further expansion, due to the actinic rays, causing an additional weight of acid to pass from B to D. A movement of the beam is thus produced which is communicated to the lever M, and is registered by means of a pen on a strip of curve-paper rotating on the drum N. The capacity of the bulbs A and E in this case is 273 cub. centim., that of G and C 319 cub. centim.; the tubes B and F and D and H are 15 centim. long and 1·25 centim. in diameter, the distance between the two sets of tubes being 35 centim. It is found desirable that the movement of the bulbs and beam should be as small as possible, and that the effect should be magnified by increasing the length of the lever. After the apparatus is blown together, care must be taken to remove all moisture by drawing dry air through the bulbs and tubes. The apparatus is exposed out of doors in a wooden box G, fig. 4, Plate III., the bulbs A and B passing through a hole in the

lid; these are protected by a glass shade, C, 45 centim. high and 20 centim. in diameter; this is mounted in a block of wood, D, so that when the box is in its normal position, the axis of the cylindrical shade points to the pole star. The two bulbs occupy a position equidistant from the sides of the shade and about midway between the top and bottom. The tube E serves as a ventilator and should be at least 2·5 centim. in diameter; the cap F protects the opening from rain, &c. The bulbs were tested, when filled with dry air, by exposing them to intense sunlight: it was found that, although the temperature within the shade varied from 15° to 40° C., the compensation was so complete that practically no deviation from the straight line, described by the pen on the drum, was observed; the bulbs were fixed to the beam (as nearly as possible over the point of support) by means of cylinders of thin sheet brass, which were slipped over the acid tubes and screwed to the beam. The instrument was calibrated by means of the silver-chloride actinometer. The result of six series of observations gave the following values in intensity units corresponding to one division on the curve-paper :—

1	series	mean of	11	observations	·021
2	"	"	14	"	·029
3	"	"	10	"	·043
4	"	"	15	"	·025
5	"	"	9	"	·033
6	"	"	10	"	·029
				Mean =	·030

In the following table the observation from one of the series is given :—

Intensity (unit = 1).	Value of 1 division in light units.	Recalculated intensity from curve.	Time of day in hours.
0·3758	0·0385	0·2925	10·75
0·1022	0·0328	0·1275	10·8
0·1693	0·027	0·1875	11·0
0·3240	0·035	0·2475	11·08
0·3348	0·029	0·3375	11·5
0·3210	0·030	0·3150	11·6
0·4457	0·039	0·3375	11·65
0·0779	0·017	0·1350	12·3
0·1087	0·021	0·1500	12·6

The curve from which the data are obtained is shown in fig. 1, Plate IV., the value of one division being equal to $\frac{1}{10}$ of a square therein represented.

In fig. 2, Plate IV., the results obtained above are multiplied by 100 and represented graphically. Fig. 3 represents the curves registered by the recording apparatus under varying conditions of light from June 13 to 16. It will be seen that such an instrument as this will record continuously the actinic intensity of the light under all conditions of weather throughout the year, and requires no attention further than winding the clock whereby the motion of the drum is maintained.

XXVI. *The Influence of Surface-Loading on the Flexure of Beams. By* Prof. C. A. Carus Wilson *.

[Plate V.]

The practical treatment of the problem of beam-flexure at the present time is based on the hypothesis enunciated by Bernoulli and Euler †, that the bending-moment is proportional to the curvature.

This assumes that the cross sections remain plane after flexure and neglects the surface-loading effect.

Saint-Venant has shown ‡ that the first assumption is untenable; but that, neglecting the surface-loading, Bernoulli's results are strictly true for one particular case of loading, that, namely, of a beam doubly supported and carrying a single isolated load, where, although the cross sections are distorted, the central displacement is zero.

I propose in this paper to describe some experiments made with a view to determining the actual state of strain in a beam doubly supported and centrally loaded, the surface-loading effect being taken into account.

The method of investigation adopted is based upon the following assumptions:—

* Read June 26, 1891.

† Todhunter and Pearson's 'History of Elasticity,' vol. i.

‡ Pearson's 'Elastical Researches of Saint-Venant.'

Fig. 1.

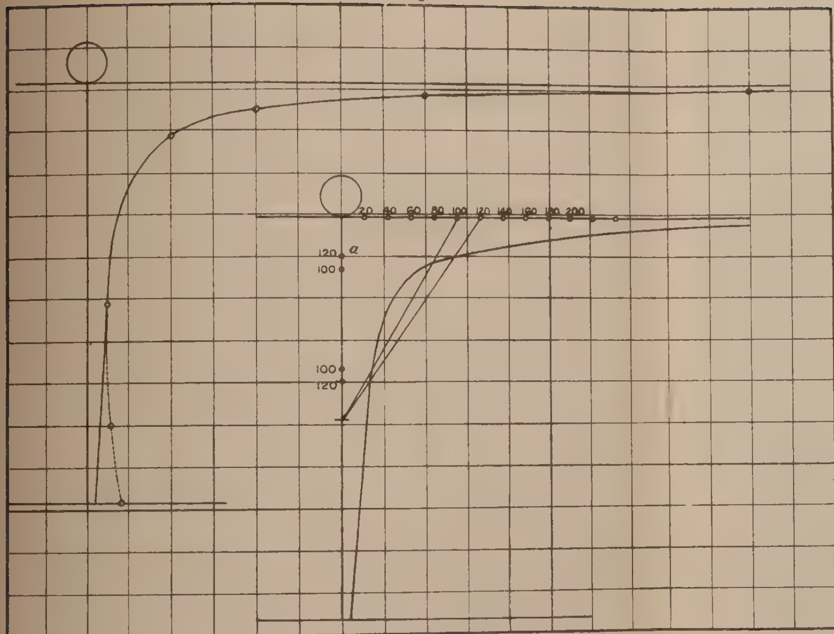


Fig. 2.

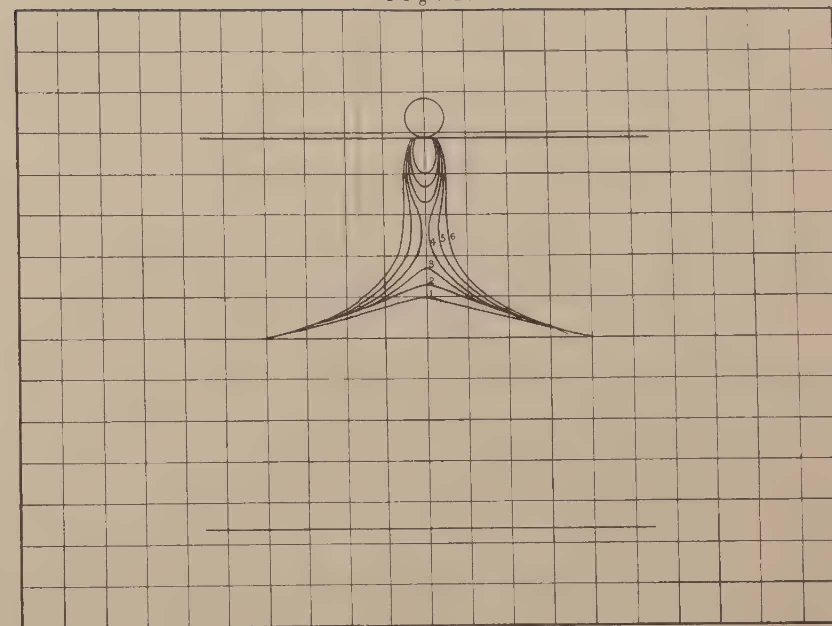


Fig. 3.

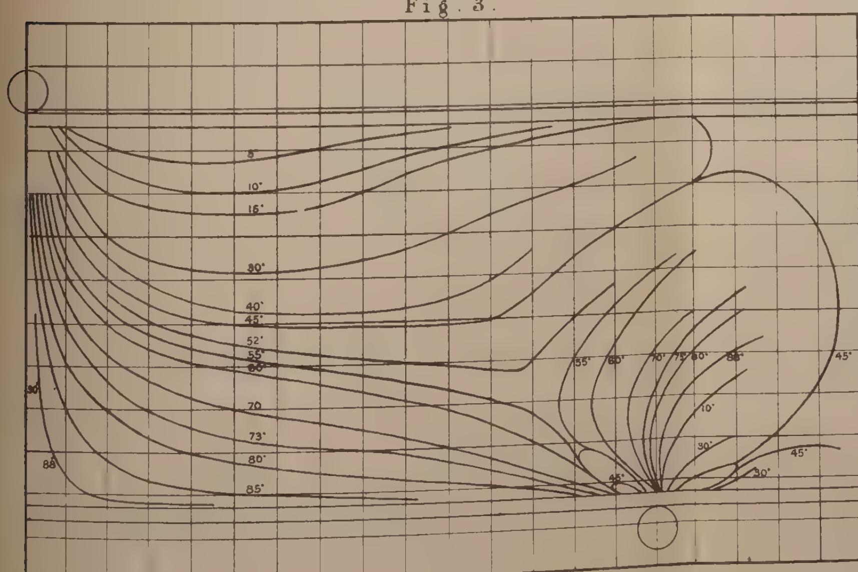
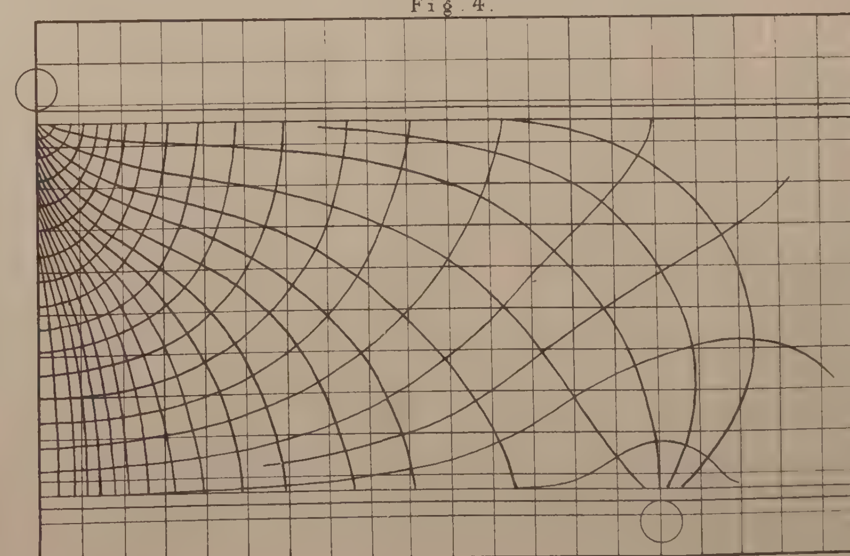


Fig. 4.



(1) The true state of strain at the centre of the beam may be found by superposing on the state of strain due to bending only, that due to surface-loading without bending.

(2) The state of strain due to surface-loading only may be found, with close approximation to truth, by resting the beam on a flat plane instead of on two supports.

(3) The strains due to bending only may be obtained from the Bernoulli-Saint-Venant results ; viz. :—

(α) The stretch for any cross section varies as the distance from the neutral axis.

(β) The central axis is unstretched.

(γ) For the same point in different cross sections the stretch varies as the bending-moment.

Saint-Venant has dealt with the shearing-strains at some little distance from the load in the case of a beam doubly supported and centrally loaded * ; and Professor Pearson has shown † that, in the case of beams continuously loaded, the results of the Bernoulli-Eulerian theory can only be considered as giving approximate formulæ when the span of the beam is not less than ten times its depth ‡.

The mathematical determination of the state of strain produced by the loading of a beam as it rests on a flat plane is one of considerable analytical difficulty.

MM. Lamé and Clapeyron have attempted the solution of a more general problem in their "Mémoire sur l'équilibre intérieur des corps solides homogènes." § The object of this paper is stated to be "to investigate the way in which the interior of a body is affected by the transmission through it of the action of forces." Here they treat the problem of a solid extending to infinity on one side of a plane, on which is a given distribution of tractive load, and also of a solid con-

* Pearson's 'Elastical Researches of Saint-Venant,' §§ 69–99.

† Pearson, "On the Flexure of Heavy Beams subject to continuous systems of Load," Quarterly Journal of Mathematics, No. 93 (1889).

‡ Rankine assumed that the surface-loading effect might be neglected. See his 'Applied Mechanics,' § 311.

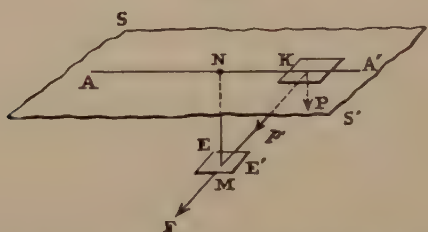
See also *Résumé des Leçons* &c. by Navier (Paris: Dunod, 1864), vol. i. p. 41 :—"Observation sur le mode d'application et de distribution des forces qui font fléchir," where the same assumption is made.

§ Crelle's *Journal*, vol. vii. p. 145 *et seq.*

tained between two parallel infinite planes. They obtain as a result a set of definite integrals giving the displacements, introducing a function involving the distribution of tractive load, from which the stresses may be deduced, but concerning which they add: "Les formules précédentes, pour être obtenues en séries numériques et immédiatement applicables, exigent la connaissance des valeurs d'un genre particulier d'intégrales définies, dont il ne nous paraît que les géomètres se soient encore occupés."

The most successful attempt at a solution of this problem is to be found in a more recent work by Professor Boussinesq, published in 1885*. The following is a brief account of the results obtained.

Fig. 1.



S S' being the surface of the solid (infinite below in length, width, and depth), M a point within, situate at a distance M N $=x$ below the surface, K any element of the surface, situate at the distance K M $=r$ from the point M, and subject to a given exterior pressure K P $=p$, having the component K P' $=p'$ along K M, the pressure which a plane element E E' taken through M parallel to the surface S S' will support, per unit of area, in consequence of the pressure p, will be found directed along the direction of K M produced, and will be equal to

$$\mathrm{MF}=\frac{3p'x}{2\pi r^3}. \quad \dots \dots \quad (1)$$

* *Application des Potentiels à l'étude de l'Equilibre et du Mouvement des Solides élastiques* (Gauthier-Villars, Paris, 1885).

See also *Théorie de l'Elasticité des Corps solides*, Clebsch; translated and annotated by MM. de Saint-Venant and Flamant. (Paris: Dunod, 1883, p. 374, note to art. 46.)

If, as a particular case, the pressure $KP=p$ be normal, then $p'=p\cos NMK=p\,\frac{x}{r}$, and

$$MF=\frac{3px^2}{2\pi r^4}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If, further, it is required to find the vertical component of MF, we have $(MF)\,\frac{x}{r}$, or

$$\frac{3px^3}{2\pi r^5}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The treatment of this particular problem is not pursued any further in this work; but Professor Boussinesq has kindly furnished me with a solution more nearly applicable to the case in point, and one which will be found to agree closely with the experimental results I had previously arrived at, and which are given later on.

Suppose there to be a uniform pressure p exerted over every element du of bearing-surface between two extremities A, A′ (see figure), having abscissæ $u=-NA=-a$, $u=NA'=+a$, and let $p=Pdu$, calling P the constant exterior pressure per unit of length $AA'=2a$.

The total pressure over unit of surface of an element E E′ will be, from equation (3),

$$\Sigma\frac{3px^3}{2\pi r^5}=\int\frac{3Px^3du}{2\pi r^5}=\frac{3Px^3}{2\pi}\int_{-a}^{+a}\frac{du}{r^5}$$

$$=\frac{3Px^3}{\pi}\int_0^{+a}\frac{du}{(x^2+u^2)^{\frac{5}{2}}}\;[NK=u].$$

Putting $\frac{u}{x}=\alpha$, $du=x\,d\alpha$, we get as the normal pressure per unit of area on E E′,

$$\frac{3P}{\pi x}\int_0^{\frac{d}{x}}(1+\alpha^2)^{-\frac{5}{2}}d\alpha\,;$$

or, very nearly, if x is much smaller than a,

$$\frac{3\mathrm{P}}{\pi x}\int_0^\infty (1+\alpha^2)^{-\frac{5}{2}}\,d\alpha.$$

The value of the integral is $\dfrac{2\alpha^3+3\alpha}{3(1+\alpha^2)^{\frac{3}{2}}}$ or $\dfrac{2+\dfrac{3}{\alpha^2}}{3\left(1+\dfrac{1}{\alpha^2}\right)^{\frac{3}{2}}}$, which,

between the limits $\alpha=0$ and $\alpha=\infty$, becomes $\frac{2}{3}$. Thus the pressure per unit of area on an element E E′ becomes $\dfrac{2\mathrm{P}}{\pi x_1}$, or

$$0{\cdot}64\,\frac{\mathrm{P}}{x}. \qquad \ldots \qquad (4)$$

This expression has the form of that given below, though, inasmuch as the problem is not altogether the same as that treated experimentally*, a difference in the coefficients is only what might have been expected.

The value of the integral between the limits $\alpha=0$ and $\alpha=\infty$ is, as has been stated, $\frac{2}{3}$, or 0·667. For $\alpha=5$, *i. e.* for $u=5x$ as the upper limit, the integral = 0·666, and for $u=2x$ the integral = 0·656; so that this solution is approximately correct for elements lying at a distance of $\frac{1}{4}$ the width of the beam from the point of contact.

Hence for a beam where the length A A′ is 5·5 millim., this solution would be applicable up to points lying at a distance of about 1·4 millim. from the top surface.

I have investigated the law up to within 0·5 millim. of the top surface, and find it to be

$$y=0{\cdot}726\,\frac{\mathrm{P}}{x}.$$

The investigation of the state of strain in glass beams by means of polarized light was first suggested by Sir David Brewster †, and his experiments are usually quoted as proving

* The mathematical solution assumes the length of bearing A A′ on an infinite surface.

† Phil. Trans. 1818, p. 156.

the truth of the Bernoulli-Eulerian theory of flexure. It is, however, easy to show experimentally that these experiments must have been made under conditions where the surface-loading effect was inappreciable; though very accurate reasoning on this point is impossible, as the drawings accompanying Sir David Brewster's paper are not to scale, and the span of the beams and the precise method of application of the loads are not indicated.

M. Neumann developed a theory of the action of strained glass in the polariscope*, and found that the velocity of light in a medium is increased by compressing it. He bases his calculations on the measurement of the deflexions of glass beams supposed to obey the Bernoulli-Eulerian theory; the beams are doubly supported and centrally loaded, having the proportions $66 \times 8{\cdot}5 \times 2$, the latter being the depth. It is not in all cases stated what spans were employed, so it is impossible to say how far the results were influenced by surface-loading.

Professor Clerk-Maxwell † has examined the state of strain in pieces of unannealed glass of various shapes, the lines of equal intensity of strain being deduced from the isochromatic lines.

The lines of Principal Stress are found from those of Equal Inclination in the manner described later on in this paper.

It has already been pointed out ‡ that "Neither Neumann nor Maxwell seems to have remarked that the difference of the velocities of the ordinary and extraordinary rays depends solely on the maximum slide of planes perpendicular to the wave-front."

An important work on this subject is found in a paper by Dr. John Kerr §. He establishes the fact that "If a plate of glass, compressed or extended in one direction parallel to its faces, be traversed normally by two pencils of light, which are polarized in planes respectively parallel and perpendicular to the direction of strain, then both pencils are retarded by

* *Abhandlungen der k. Akademie der Wissenschaften zu Berlin*, 1841, vol. ii. pp. 50–61.

† Trans. Roy. Soc. Edinburgh, vol. xx. (1853) p. 117.

‡ Hist. of Elasticity, vol. i. p. 643.

§ Phil. Mag. October 1888.

the strain in the case of compression, and both are accelerated by the strain in the case of tension." Also that "strain-generated retardations, absolute as well as relative, are sensibly proportional to the strain," thus confirming Wertheim's results.

Dr. Kerr employs in his experiments a bent glass beam, doubly supported and centrally loaded, having the ratio of span to depth* of 8·4 to 1, and assumed to obey the Bernoulli-Eulerian theory.

I would draw attention to the disagreement between the results arrived at by M. Neumann and Dr. Kerr, the former stating that the velocity of light in a medium is increased by compressing it, while the latter states that the velocity is diminished.

Dr. Kerr examined a beam having a span equal to 8·4 depths, and at a point where the surface-loading effect would be least; whereas M. Neumann examined a beam—span to depth ratio not stated—immediately under the load.

I can only attempt to account for the discrepancy by pointing out that if the span is diminished to less than four depths, the elements of glass that M. Neumann assumed to be in a state of squeeze are actually, as will be shown later, in a state of stretch.

The instrument with which the following experiments were made consists of a steel straining-frame in which the beam to be examined is placed; the beam rests—for flexure—on two steel rollers, and is loaded by a micrometer-screw which bears on a third central roller. The base of the frame is divided, from the centre, in divisions of 2 millim. so that the supports can be set for any required span. A micrometer-screw is placed in the base of the frame opposite the load, so that deflexions can be measured to one ten thousandth of an inch. Two screws in the two sides of the frame enable lateral pressure to be applied. The whole frame can be moved in any direction in its own plane, so that all parts of the beam may be examined. The optical arrangements consist of two nicols, of which the upper is provided with a graduated disk on which the angle of rotation can be observed; a microscope

* According to the figure.

with micrometer-eyepiece can be fitted when it is desired to measure the fringes; circularly polarized light can be used when required.

The beams used were marked on one side with 2 millim. squares; they were covered with paraffin and marked in a dividing-engine and then etched; the lines thus formed enabled the position of dark bands to be determined with accuracy.

Proposition I.

If a beam of glass be laid on a flat surface and loaded across its upper surface, the shear at any point on the normal at the point of contact of the load is inversely proportional to the distance from the point of contact.

Experiment 1. A beam of annealed glass 61 millim. × 6·5 millim. × 20 millim. deep was placed in the steel straining-frame with its narrow side resting on a piece of thin paper.

A steel roller 2 millim. in diameter, 10 millim. long, was placed across the middle of the top surface and loaded by the screw.

The nicols were crossed and at 45° to the axis of the beam.

A quarter-wave mica plate was placed between the beam and the analyser, with the plane containing the optic axes at right angles to the length of the beam.

At that point *a* on the normal where the difference of phase between the ordinary and extraordinary pencils traversing the beam is equal and opposite to the difference of phase produced by the mica plate—the effect will be as if there were no strained glass between the two nicols, and there will therefore be a black spot as the nicols are crossed.

The position of this spot on the normal is plotted on a sheet of squared paper, and an ordinate parallel to the axis chosen to represent the shear.

A second quarter-wave plate is now superposed on the first, and the black spot consequently moves up the normal to where the shear is twice what it was at *a*; this point, *b*, is noted, the second mica plate removed, and the load reduced until the black spot with one mica plate is brought to *b*. In this way a series of points *a*, *b*, *c*, *d* on the normal are found at any one of which the shear is twice what it is at the point below.

Now it is proved later on that the strain at any point varies as the load on the beam; hence by taking the ordinate at b twice that at a, at c four times, and at d eight times, and so on, we get points on the curve of loading along the normal for the load that give a difference of phase at a equal to that of one-quarter wave-plate.

The results are plotted on Plate V. fig. 1: the observed points are indicated by circles, through one of which an hyperbola has been drawn taking the normal and the upper surface of the beam as asymptotes.

It will be seen that the six upper circles lie very nearly on the hyperbola.

It is clear that the upper surface of the beam is an asymptote only when the surface of contact between the beam and the roller is a line—making the stress there infinite; but in practice this cannot be so, the smallest pressure giving a bearing surface—as the roller indents the beam—making the stress there finite, *i. e.* the asymptote will be at some finite distance θ, say, above the point of contact, and θ will vary with the load. I have calculated below that with a load of 115·3 lb. on this same beam, the value of θ is 0·044 millim.

The apparently irregular position of the two lower points observed indicates the amount of error made in the assumption (2) above that the surface-loading effect may be found by substituting a flat plane instead of two supports.

This assumption would be correct only if the beam were of infinite depth and the surface-loading effect of the support infinitely small; here, however, the steel frame itself produces a surface effect, and this, added to that due to the load, makes the points observed lie off the hyperbola, which would be the true curve (as drawn) if the beam were of infinite depth.

The effect of the steel frame must be very small compared with that due to the load for points in the upper half of the beam. In drawing the hyperbola I have considered it as negligible at the centre of the beam; in other words, I consider that the correction of the position of the six upper points, required to allow for the surface effect of the frame, would not make them deviate seriously from the hyperbola.

It must be noted, however, that when the beam is resting on two supports the surface effect of the frame disappears,

since the beam only touches the supports and surface effect can only be caused by actual contact; hence I conclude that the surface effect due to loading only is strictly represented by the hyperbola and is as if the beam were of infinite depth*.

In order to establish the hyperbolic law with greater certainty, experiments were made enabling as many as seven points on the curve to be obtained within 3·5 millim. of the point of contact, the highest point being about ·5 millim. from the top of the beam.

Within this range the effect due to the steel frame may with accuracy be neglected.

Experiment 2. A beam of annealed glass, 61 millim. × 6·5 millim. × 20 millim. deep, was placed in the steel straining-frame, on a piece of thick paper, and loaded as before with the steel roller 2 millim. in diameter.

Nicols crossed and at 45° to the axis of the beam.

The screw load was applied until six interference-fringes appeared under the roller; these were examined through a microscope with a micrometer-eyepiece divided to thousandths of an inch. Light from a sodium-flame was used, and the distance between the point of contact and the intersection of each fringe with the normal was measured in micrometer-divisions.

I. Distances in micrometer-divisions to successive fringes:

11·0 13·5 17·0 23·0 35·0 71·5,

but the shears are as 6 . 5 . 4 . 3 . 2 . 1, since there is a difference of phase of only $\frac{1}{2}$ a wave-length required to produce a fringe, hence taking the products of distances into shears we get

66·0 67·5 68·0 69·0 70·0 71·5.

But we have so far neglected the value of θ, the distance of the axis of shears from the point of contact.

By taking the two most reliable observations, where the distance from the point of contact is large and yet where the

* According to this reasoning there appears to be a shear of finite amount at the bottom of the beam—when doubly supported—due to loading only, but this does not seem to me to be inconsistent with the surface conditions.

fringes are well defined, we should have, if the law is hyperbolic,

$$3(23+\theta)=4(17+\theta),$$

or

$$\theta=1^{*}.$$

Correcting the original readings by adding θ to each, we get

12 14·5 18 24 36 72·5,

and the products become

72 72·5 72 72 72 72·5.

II. Same beam, &c., as before, roller and load readjusted. Distance to successive fringes :—

11·5 14·25 17·75 24·0 36·0 75·0

To find θ, take

$$3(24+\theta)=4(17{\cdot}75+\theta), \text{ or } \theta=1.$$

Correcting the distances, we have

12·5 15·25 18·75 25·0 37·0 76·0,

and the products of the distances into the shears become

75·0 76·25 75·0 75·0 74·0 76·0.

III. Same beam, &c., as before, roller and load readjusted. Distance to successive fringes :—

10·75 12·5 15·25 19·25 26·0 39·0 80·5.

To find θ take $3(26+\theta)=4(19{\cdot}25+\theta)$, whence $\theta=1$.

Correcting the distances, we have

11·75 13·5 16·25 20·25 27·0 40·0 81·5,

and the products become

82·25 81·0 81·25 81·0 81·0 80·0 81·5.

The law of variation of shear along the normal is thus hown to be hyperbolic.

* One micrometer-division = 0·044 millim.

Experiment 3. The steel straining-frame was removed from the instrument and—by a screw inserted in the place of the straining-screw—hung from a balance, which could be loaded with shot and had a leverage of 50 to 1: a steel stirrup was hung over the frame with two hardened points resting on the two guiding-pins; one lower end of the stirrup was secured to the body of the balance, the beam inserted and balanced, and shot put in the pan. This lifted the straining-frame and loaded the beam.

Beam [B] 56 millim. × 20 millim. × 6·5 millim. placed on the base of the steel frame on a piece of thin paper: loaded by a steel roller 2 millim. in diameter. Viewed through nicols crossed and at 45° to the horizontal axis of the beam.

The balance was loaded until the first blue fringe was brought down to a given position on the beam, and the weight of shot observed; the same fringe was then brought down to a lower given position, and the weight of shot again observed, and so on for successive points.

Distance (α) of given points on normal from top of beam, in millim.	Load (β) on roller (weight of shot) × 50 in lb.			β/α.
	1.	2.	Mean.	
1·15	40	39	39·5	34·34
3·2	114	105	109·5	34·22
4·2	145	149	147	35·00
5·2	182	180	181	34·80
6·2	218	......	218	35·16

If the shear at 4·2 millim. with 147 lb. be taken as unity, the shear at 5·2 millim. with this same load will be $\frac{147}{181}$, since the same shear is produced at 5·2 millim. with 181 lb. as is produced at 4·2 millim. with 147 lb. Hence if the curve of loading is an hyperbola, we should have

$$4{\cdot}2 \times 1 = \frac{147}{181} \times 5{\cdot}2 \text{ or } \beta/\alpha \text{ a constant.}$$

From the third column given above the values of β/α will be seen to be nearly equal in each case; the value of θ has here been neglected; if we put $\theta = 0{\cdot}04$ millim., the values of β/α become

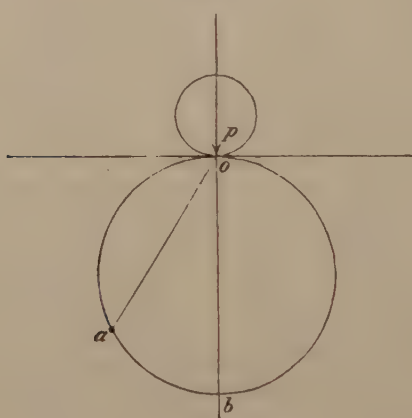
34·6 33·8 34·7 34·5 34·9.

Proposition II.

Things being arranged as in Proposition I., it is required to determine the locus of points of equal intensity of shear, and to show that at any point whatever the shear is inversely proportional to its distance from the point of contact.

Experiment 4. The beam was examined under circularly polarized light, as in Clerk-Maxwell's experiments, in order to obtain the variations in the amount of the strain uncomplicated by variations in the directions of the principal stress-axes; white light was used.

Fig. 2.



The loci of points of equal shear were found to be circles, as in the figure; circles of equal shear were obtained up to 8 millim. diameter with this beam.

Hence the shear at any point a equals the shear at b, if $o\,b\,a$ is a circle, and $o\,b$ the normal at o; *i.e.* shear at a

$$=k\frac{p}{ob}=k\frac{p\cos\theta}{o\,a},$$

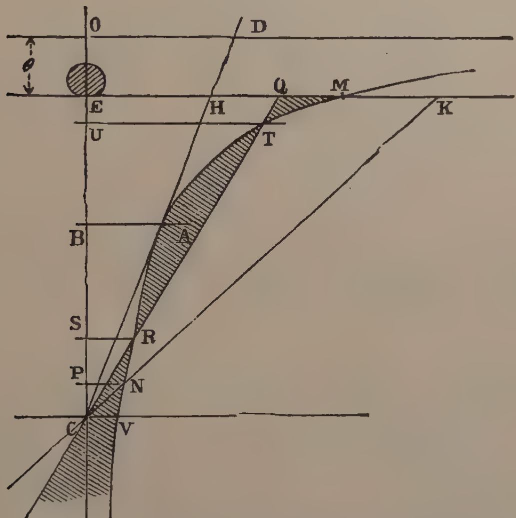
k being some constant, but $p\cos\theta$ is the resolved part of the pressure at o* along $o\,a$; hence the shear at any point is inversely proportional to its distance from the point of contact.

* See Professor Boussinesq's results quoted already.

Proposition III.

The state of strain at the centre of the beam when doubly supported may be found by superposing on the state of strain

Fig. 3.



due to bending only, that due to surface-loading without bending.

It has been proved that the state of strain along the normal at the point of contact due to the surface-loading may be represented by an hyperbola whose asymptotes are respectively the normal itself and a line parallel to the axis of the beam at a distance θ from the point of contact. Let O C, O D in fig. 3 represent these asymptotes, O E $= \theta$; let an hyperbola be drawn whose ordinates parallel to O D represent the shear at any point along E C for a given load: since the shear is proportional to the compressive stress, these ordinates may be considered as proportional to the compressive stress at any point along E C.

By our (α) assumption we may represent the stresses at any point along E C, due to bending, by a right line drawn through C, the centre of the depth.

Let C K be such a line, drawn on the same scale as the hyperbola, so that E K represents the shear (vertical stretch)

at E due to bending *, while E M represents the shear (vertical squeeze) due to loading.

These two curves must intersect at some point N; at the corresponding point P on the normal the shear (vertical squeeze) due to the loading is equal to the shear (vertical stretch) due to the bending: an element of volume at P will therefore be subject to voluminal compression only, and the shear will be zero, there will therefore be no birefringent action, and when viewed with crossed nicols there should be a dark spot on a white field.

If the load is kept constant and the span diminished, E K will decrease until C K cuts the hyperbola at a second point; we should now get two points of darkness. As the span is still diminished these dark points should rise and fall respectively until they coincide, when C K is a tangent to the hyperbola; after this they should separate out at right angles.

Plate V. fig. 2 gives the results of an experiment (5) made with constant load and varying spans. The beam was 128 millim. × 19 millim. deep × 5·5 millim. thick, supported on two steel rollers 2 millim. in diameter and centrally loaded over a similar roller: the nicols were crossed and at 45° to the axis. The following table gives the spans:—

Curve.	Span in millim.	Ratio of span to depth.
1	120	6·31
2	100	5·26
3	88	4·63
4	80	4·21
5	78	4·10
6	72	3·79

This experiment shows that there are, generally, two points

* The compressive stress due to bending, at any point on C E, produces a shear (vertical stretch) and a voluminal compression, and both are proportional to the stress, similarly for the shear (vertical squeeze) and voluminal compression produced by the stress due to the loading; so for this purpose it is indifferent whether the ordinates of the two curves represent the compressive stresses or the shears produced.

of zero shear which close up as the span diminishes and then open at right angles.

The same phenomena may be observed by placing a beam on a flat surface and loading it, and then placing over this beam a second, which may be bent with a very long span, or by two couples at the end; the effect is the same for different degrees of bending as for varying spans in the former experiment.

Thus for spans of four to five depths the normal under the load is divided into three parts by two points of zero shear, elements between these points being subject to shear (vertical stretch), while elements above and below them are subject to shear (vertical squeeze).

When, however, the span is less than four depths, every element in the cross section under the load is subject to shear (vertical squeeze) and the greatest strained element is immediately under the load.

These results may be further checked and confirmed by examining each part of the normal by placing over it a beam bent in the hand; if the part under examination is in shear, say (vertical squeeze), darkness may be produced by superposing a part of the second beam oppositely strained; if the strains were similar, increased brightness would result.

I exhibit also the results of experiments made to determine the position of the black bands for lower ratios of span to depth.

The dimensions of the beam were 124 millim. × 20 millim. deep × 6·5 millim. thick, loaded on rollers like the others; nicols crossed and at 45° to the axis.

Here the effect of the supports is very marked, so that when $p=2$ the black band only just touches the axis.

It must be remembered that at the point where the black band cuts the normal the shear is zero, but that everywhere else on the band all that is indicated is that the directions of resultant tension and compression are at 45° to the axis of the beam.

Experiment 6 was made to establish Proposition III. with greater certainty.

Beam 128 millim. × 19 millim. × 5·5 millim. was placed on the base of the straining-frame, on a piece of thin paper

and loaded with shot until the first blue fringe came down to a point 1·7 millim. from the top. The load was 65 lb.

The same beam was then supported on two steel rollers 2 millim. in diameter and 120 millim. apart, and centrally loaded over a similar roller until the same blue fringe appeared at the bottom of the beam. The load was 55 lb.

An hyperbola has been drawn (see fig. 1, Plate V.) of convenient proportions, cutting the horizontal through the abovementioned point at 28·5 divisions from the normal; the shear corresponding to the blue fringe is thus represented by 28·5 divisions, and there is that shear at the point with a load of 65 lb.

Now the stress due to bending, at the extreme bottom fibre of a beam 19 millim. deep, 120 millim. span, and 5·5 millim. thick, with a load of 55 lb., is 1·436 tons per square inch.

The vertical compressive stress at this point, due to the load of 55 lb., is, as is shown later on, 0·121 ton per square inch; but we are not justified in superposing the shears produced by these two stresses, being tensile and compressive at right angles, and the former as much as twelve times the latter, so I shall take the stress at the blue fringe as 1·436 tons per square inch.

Hence the compressive stress produced by 65 lb. over a span of 120 miliim., at the top fibre, is

$$1{\cdot}436 \times \frac{65}{55},$$

and the corresponding value in scale-divisions is

$$1{\cdot}436 \times \frac{65}{55} \times \frac{28{\cdot}5}{1{\cdot}436} = 33{\cdot}7 \text{ divisions.}$$

This distance is set off along the top surface in the figure, and the point so found joined to the centre of the middle section: where it cuts the hyperbola we should get darkness on the normal with a span of 120 millim. We can also draw lines representing the bending-stresses for other spans for the same load of 65 lb.

The position of the black bands on the normal, as found by experiment for spans of 120 and 100 millim., are indicated on the normal, and will be found to agree very closely with those

points found independently by the intersection of the two curves.

The curve of bending-stresses is a tangent to the curve of loading at a span of 73 millim., as measured from the figure, whereas it is apparently 82 millim. when actually observed ; it would appear more correct to determine this span by drawing the curve through two points which can be observed with accuracy, and then drawing the tangent and measuring the intercept, since the experimental determination of the span giving coincidence of the two dark bands is one liable to considerable error.

By drawing lines from the centre to the points along the top surface corresponding to longer spans we see that the deviation of the so-called "neutral axis" from the centre is considerable : thus even at a span = 10 depths = 190 millim. it should be 1 millim. above the centre.

Proposition IV.

The strain at every point along the normal due to loading varies directly as the load.

Experiment 7. The beam is placed on two supports as before, with a small central load, and the points of intersection of the black bands with the normal are noted. The load is now increased up to the safe limit when the points of intersection are observed to remain unaltered.

We know that the strain at any point on the normal due to bending is proportional to the load ; hence if the point of intersection of the curves of bending and loading remains the same when the load is increased, we know that the strain at any point due to the loading must vary as the load.

Proposition V.

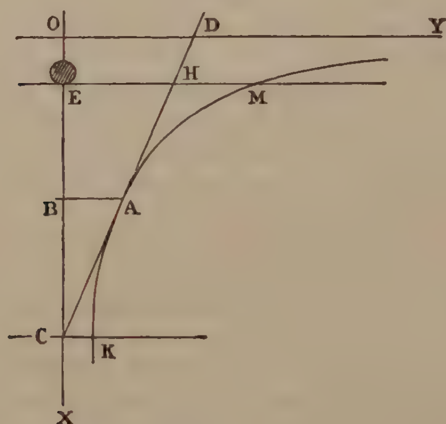
To determine the constant in the equation to the curve of loading along the normal for any beam.

Let O X represent the vertical through the centre of a beam centrally loaded, E the point of contact of the load with the top of the beam E K ; O Y the axis of shear, $OE=\theta$; K A M the hyperbola of loading for any given load, C A H D

the line of stresses due to bending along C E, for the same load, the span being chosen so that C A H is a tangent to the hyperbola at A; *i. e.* so that the dark bands coincide at B. Then O Y and O X are the asymptotes of the hyperbola.

It has been proved that the equation is of the form $y=k\frac{1}{x}$, where y is the compressive stress at a point on the normal E C at a distance X from O. If W is the load and b

Fig. 4.



the width of the beam = length of bearing of loaded roller, we have

$$y=k.\frac{W}{bx}\text{—for the given beam.}$$

Then $O\,D = B\,A = 2k\frac{W}{bx}$ (since B A represents the stress at B due to the load W). Also $E\,H = \frac{3}{2}\frac{Wl}{h^2b}$, where E H represents the stress at E due to a load W on a beam of depth h and width b and span l; and

$$\frac{EH}{OD}=\frac{CE}{CO};\quad \therefore\ CO=CE\,\frac{OD}{EH},$$

$$\text{or } \frac{h}{2}+\theta=\frac{h}{2}\times 2k\frac{W}{bx}\times\frac{2}{3}\frac{h^2b}{Wl}=\frac{2}{3}\cdot\frac{h^3k}{lx}$$

$$=\frac{2}{3}\frac{h^3k}{\frac{l}{2}\left(\frac{h}{2}+\theta\right)} \text{ [since } x=\tfrac{1}{2}\text{CO] ;}$$

$$\therefore \left(\frac{h}{2}+\theta\right)^2=\frac{4}{3}\frac{h^3k}{l};$$

$$\therefore \quad \frac{1}{4}+\frac{\theta}{h}+\frac{\theta^2}{h^2}=\frac{4}{3}\cdot\frac{h}{l}\cdot k; \text{ put } \frac{l}{h}=\rho;$$

$$\therefore \quad k=\frac{3}{4}\rho\left(\frac{1}{4}+\frac{\theta}{h}+\frac{\theta^2}{h^2}\right).$$

To find k the beam is placed on two supports and centrally loaded ; the two points where the black bands cross the normal are observed (the span being longer than four depths), and plotted, and an hyperbola drawn through them ; a tangent is then drawn to this curve from the centre of the section and its intercept on the upper edge measured, the span giving coincidence of the black bands can then be calculated.

Experiment 8. For a beam 128 millim. × 19 millim. deep × 5·5 millim. I find this span to be 73 millim. ; hence

$$\rho=\frac{73}{19}=3{\cdot}84.$$

Taking θ at 0·04 millim., $\frac{\theta}{h}=$ 0·002 millim., and neglecting $\frac{\theta^2}{h^2}$, we have

$$h=\tfrac{3}{4}\times 3{\cdot}84\times 0{\cdot}252=0{\cdot}726.$$

Proposition VI.

To verify the equation to the curve of loading.

Experiment 9. Beam 128 millim. × 19 millim. × 5·5 millim.

The stress corresponding to the blue fringe with this beam was found, as already explained, by loading the beam over a span of 120 millim., until the blue fringe appeared at the bottom of the beam ; the load required was 55 lb. ; hence the corresponding stress is 1·436 tons per square inch*.

* From the equation $y=k\frac{W}{bx}$, there is a compressive stress of 0·121 ton per square inch here due to the load. I have not added the effect of this to that of the bending, as there is no proof that the superposition of small strains holds when the strains themselves are so unequal.

When laid on the base of the steel frame, the same fringe was observed at 1·7 millim. from the top with a load of 65 lb.

From the equation to the curve of loading, taking $k=0{\cdot}726$, $\theta=0{\cdot}04$ millim., we ought to have a stress at 1·7 millim. from the top equal to

$$y=0{\cdot}726\times\frac{25{\cdot}4^2}{2240}\times\frac{65}{1{\cdot}74}\times\frac{1}{5{\cdot}5}=1{\cdot}419 \text{ tons per square inch.}$$

The lines of Principal Stress afford a convenient means of studying the condition of strain in a bent beam.

In a memoir published in 1838* Lamé discussed the problem of the equilibrium of an elastic solid, and investigated the properties of what he termed "isostatic surfaces," or surfaces where only normal "actions" are applied.

In 1870 Saint-Venant† examined the differential equations to which the subject of "isostatic surfaces" gave rise, and in 1872 Professor Boussinesq‡ gave a geometric method for constructing isostatic lines passing through any given point. This memoir was shortly followed by a second §, treating of the integration of the equations involved.

Rankine has examined the form of the curves of Principal Stress, and given an expression from which the curves can be drawn ||. He neglects the surface-loading effect as "in most cases practically of small intensity when compared with the other elements of stress." On comparing his curves with those in Plate V. it will be noticed how closely the curves of tension agree, while the curves of compression are very dissimilar.

Sir George Airy has calculated and drawn the curves of principal stress for several cases of flexure, including that of a beam doubly supported and centrally loaded ¶. He assumes "that there is a neutral point in the centre of the depth; that on the upper side of this neutral point the forces are forces of tension, and on the lower side are forces of compression, and that these forces are proportional to the distances from the

* *Comptes Rendus*, vol. vii. p. 778: "Mémoire sur les surfaces isostatiques dans les corps solides en équilibre d'élasticité."

† Ibid. vol. lxx. ‡ Ibid. vol. lxxiv. p. 242. § Ibid. vol. lxxiv. p. 318.

|| 'Applied Mechanics,' §§ 310 and 311.

¶ Phil. Trans. 1863, part 1.

neutral point;" but he says "These suppositions seem to imply that the actual extensions or compressions correspond exactly to the curvature of the edge of the lamina." The surface-loading effect is not here taken into account; and it would have been interesting to compare the results as shown in fig. 6, for a beam in which the span equals ten depths, with the actual curve as found by experiment. This comparison, however, would lead to erroneous conclusions, since it has been shown* that the results arrived at are not consistent with the fundamental equations, and the form of the curves can be accepted only as a very general approximation.

Proposition VII.

To determine the lines of Principal Stress in a glass beam doubly supported and centrally loaded.

Experiment 10.—A glass beam, 128 millim. × 19 millim. deep × 5·5 millim. thick, was placed in the steel straining-frame on two steel rollers 2 millim. in diameter, and centrally loaded over a similar steel roller.

The span chosen was 60 millim., giving for ρ the value 3·15.

The nicols were crossed and set at an observed angle, and the black band plotted on squared paper corresponding to the squares on the glass beam. This band of course represents the locus of points where the axes of principal stress are parallel to the directions of the planes of the nicol.

The nicols were then turned through a small angle α, the new position of the black bands plotted, and so on for several different angles. These curves are shown in Plate V. The lines of principal stress are easily deduced from these and are shown in Plate V. fig. 4.

Since communicating the above, Sir George Stokes has gone very fully into this problem, and has kindly allowed me to quote the following extracts from letters I have received from him on the subject:—

"Let A be the point in the upper surface where the pres-

* See criticism on Sir George Airy's solution in Ibbetson's 'Mathematical Theory of Elasticity,' note on p. 358.

sure (P) is applied; B, C the points of support below, which I suppose to be equidistant from A; D the middle point of BC. Let y be measured downwards from A; denote BD or DC by a, and AB by b. You have the expressions for the stresses produced by P in an infinite solid $\left(x=\frac{2P}{\pi}\cdot\frac{1}{y}\right)$, and the question is, What system must we superpose on this to pass to the actual case? This, as I showed you, is the system of stresses produced by a system of forces applied to the surface. The forces consist—(1) of the two pressures $\frac{1}{2}$ P at B and C; (2) of a continuous oblique tension below, represented in drawing by a fan of tensions directed at every point of the lower surface from the point A.

"Imagine now the beam cut into two by a plane along A D. Consider one half only, say that on the B side. Everything will remain the same as before, provided we supply to the surface A D forces representing the pressures or tensions which existed in the undivided beam. On account of the symmetry, the direction of these must be normal.

"At D the vertical pressure on a horizontal plane in the infinite solid is compounded with an equal vertical tension due to the fan. Hence, of the vertical pressure in A D which must be superposed on the vertical pressure in the infinite solid, we know thus much without obtaining a complete solusion of the problem, namely, that it must equal minus $2P/\pi b$ at D and 0 at A. If we suppose it to vary uniformly between, we are not likely to be far wrong.

"This leads to the following expression for the vertical pressure in A D:—

$$\frac{2P}{\pi}\left(\frac{1}{y}-\frac{y}{b^2}\right).$$

"Now for the horizontal. We know that the complete system of external forces must satisfy the conditions of equilibrium of a rigid body. The direction in each element of the fan passes through A, about which therefore the fan has no moment. Hence the moment of the horizontal forces along A D taken about A must equal $\frac{1}{2}$ Pa. Again, the resultant of the semi-fan is a force passing through A, and its vertical

component is $\frac{1}{2}$P. Its horizontal component is the integral of

$$\frac{2Pb^2}{\pi} \cdot \frac{x\,dn}{(b^2+x^2)^2},$$

taken from 0 to infinity, or $\frac{P}{\pi}$.

"Hence of the horizontal forces along A D we know these two things :—

(1) The sum must equal $\frac{P}{\pi}$,

(2) The moment round A must equal $\frac{1}{2}Pa$.

"In default of a knowledge of the law according to which the force varies with y, it is natural to take it, for a more or less close approximation, to be expressed by the linear function $A+By$, or say Y. To determine the arbitrary constants A, B, we have only to equate the integral of $Y.dy$ to $\frac{P}{\pi}$, and that of $Yy.dy$ to $\frac{1}{2}Pa$, the limits being 0 to b. We thus get for the expression for the tension at any point of A D,

$$\frac{P}{b}\left(\frac{4}{\pi}-\frac{3a}{b}\right)+\frac{6P}{b}\left(\frac{a}{b}-\frac{1}{\pi}\right)\frac{y}{b}.$$

"At neutral points the vertical pressure equals minus the horizontal tension, giving

$$\left(\frac{6\pi a}{b}-8\right)\frac{y^2}{b^2}+\left(4-\frac{3\pi a}{b}\right)\frac{y}{b}+2=0\cdot$$

or, putting for shortness $\frac{3\pi a}{b}-4=m$,

$$2m\left(\frac{y}{b}\right)^2-m\frac{y}{b}+2=0, \quad \therefore \ \frac{y}{b}=\frac{1}{4}\pm\sqrt{\frac{1}{16}-\frac{1}{m}}.$$

For the neutral points to be real and different, we must have

$$m>16, \quad \frac{2a}{b}>\frac{40}{3\pi}.$$

When the neutral points coalesce into one, we have m equal 16, y equal $\frac{b}{4}$; and for the ratio of the span to the depth,

$\frac{2a}{b}$ equal $\frac{40}{3\pi}$, equal 4·245, or, say, the span is $4\frac{1}{4}$ times the depth.

"As regards the horizontal tension at points along A D, you take a linear function of y as I do, and your condition of moments is the same as my (2), but in lieu of my (1) you do what is equivalent to taking the total tension *nil*. You further omit the correction to the vertical pressure when we pass from a solid of infinite depth to one terminated by a plane below. You further take the coefficient of $\frac{P}{y}$ as k, a constant to be determined by the observations, instead of $\frac{2}{\pi}$.

"Taking the place of the neutral point (at one fourth of the depth) and the ratio of span to depth as given by my formulæ, and then treating them as if they had been the results of experiment, and substituting in your formulæ for the determination of k, I got 0·7947 instead of 0·64. The largeness of your coefficient is I think fully accounted for by the employment of the formulæ which you used.

"In your method you take the stress belonging to the solid supposed infinitely deep, and superpose it on the stress corresponding to a pure bend.

"This comes to the same thing as retaining three terms only in the equation I gave in my letter for determining the y of the neutral points.

"The equation thus becomes

$$\frac{6\pi a}{b}\cdot\frac{y^2}{b^2}-\frac{3\pi a}{b}\cdot\frac{y}{b}+2=0,$$

or

$$2m\frac{y^2}{b^2}-m\frac{y}{b}+2=0,$$

where

$$m=\frac{3\pi a}{b}\text{ instead of }\frac{3\pi a}{b}-4.$$

"When the two neutral points merge into one, we have in both cases alike y equal $\frac{1}{4}\,b$, and the only difference is that $3\pi\frac{a}{b}$ equals m instead of m plus 4.

"If you had supposed the coefficient for the infinite solid to be an unknown quantity k, and had applied your observations to determine it, using my formulæ instead of your own, you would have got something very close indeed to 0·64.

"It is noteworthy that in your problem, taken as one in two dimensions, the theoretical stresses in the planes of displacement are independent of the ratio between the two elastic constants; in other words, independent of the value of Poisson's ratio."

I have calculated the positions of the neutral points from Sir George Stokes's formula

$$\frac{y}{b}=\frac{1}{4}\pm\sqrt{\frac{1}{16}-\frac{1}{m}}$$

for spans of 88, 100, and 120 millim. in a beam 128 millim. long × 5·5 millim. wide × 19 millim. deep. These are given in the following Table in the 2nd and 3rd columns. The results of actual observations (see p. 192) are given in columns 4 and 5; while columns 6 and 7 give the same points as found by plotting the intersection of the curves of pure bending and loading (infinite solid assumed) :—

Span.	Distance of Neutral Points from top edge, by					
	Sir George Stokes's formula.		Observation.		Intersection of curves.	
88.........	6·3	3·2	6·4	3·3	6·9	2·7
100.........	7·0	2·5	7·2	2·5	7·3	2·3
120.........	7·7	1·8	7·8	1·8	7·8	1·75

The error by the intersection method is greater in proportion as the span is smaller, as might have been expected.

If the observed positions of the neutral points are inserted in Sir George Stokes's formula, the value 0·64 is obtained for the constant k in the equation $x=\frac{2P}{\pi}\cdot\frac{1}{y}$.

M'Gill University, Montreal,
October 12, 1891.